A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

MAY, 1801.

ARTICLE I.

Observations and Experiments on the Galvanic Power. By a Correspondent.

To Mr. NICHOLSON.

SIR, *Edinburgh.*

THE readiness with which you admitted a former communication of mine (notwithstanding that its deficiencies must have been apparent to your acuteness, and that there are now several parts of it which later experiments would induce me to correct or explain), has encouraged me to lay before you some phenomena that I have observed in the interesting science of Galvanism.

I began my inquiries on that subject almost immediately after the first publication of the account of Signor Volta's Pile in your Journal, in concert with a friend, who, to the experience of years, added all the ardour of youth in the pursuit of science, but whose life was shortened by an unfortunate accident in a vigorous old age: I premise this account of my own experiments, by rescuing from oblivion the few observations that his time and health would allow him to make; and which will come with no small interest to the world, when it is known they were those of the intimate friend of a Franklin and of a Black.

His firſt attempts led him to compare Galvaniſm with the old electricity; and he was decidedly of opinion, that there was a ſufficient diſtinction betwixt them to entitle the former to the rank at leaſt of a new ſpecies; this obſervation my own experience has every day confirmed, eſpecially ſince I have been able to give ſo very conſiderable a degree of ſtrength to the pile as I have latterly done.

He attempted to uſe reſin as a lining for a white iron caſe to the pile, in which, after ſtanding for a ſhort period, its power ſtopped; and upon examining the pieces he found the reſin completely diſſolved into a ſubſtance reſembling turpentine, and forcibly drawn betwixt the ſilver and zinc; this ſhows a violence of action which has never as yet been obſerved in Galvaniſm, and may ſerve as a caution to others againſt uſing reſinous ſubſtances with the pile, at the ſame time that it opens a field of inquiry with regard to its effects on that matter.

His next and moſt important obſervation was made on a moſt ſingular phenomenon, which he perceived on the ſides of the ſilver and zinc pieces which were in immediate contact, when ſtrong paſteboard, moiſtened with a ſolution of common ſalt, was uſed for ſetting up the pile: the pieces of paſteboard were of a polygonal form, and ſmaller than the metallic pieces; their figure was exactly ſtamped in a black colour, not on the ſides of the pieces in contact with them, but on the dry metallic faces which were in contact with metal only; this very ſingular appearance, which I had frequent opportunities of examining, conveys an idea that ſomething, circumſcribed in its action by the figure of the moiſtened diſks, and capable of penetrating through the pores of the metals and of oxidating them, formed a conſtituent part of the Galvanic fluid.

He alſo obſerved, what might be readily expected, that people advanced in years felt the ſmall ſhocks of Galvaniſm leſs readily than younger perſons; but what was ſingular, they felt the ſtrong ſhocks more ſeverely; this may throw ſome light on the nervous fluid, and the change that it undergoes by age.

He found that when the experiment of decompoſing water was made in an open glaſs, that the water, although kept for a week after, unconnected with the pile, and a ſufficient quantity of ſalt had been added, to prevent all ſymptoms of putrefaction, yet continued to emit a gas, and a mucous matter was precipitated from it, of a whitiſh colour; this ſeems to have ſome connection with an extraordinary liquor, procured by the ingenious Dr. Moyſe, by evaporating the waſtings of the pile; but the account of which I by no means wiſh to anticipate, as it is to be hoped that the learned Doctor will favour the world with it himſelf.

You will no doubt regret that ſo truly philoſophic an obſerver ſhould be loſt to mankind, and had you known the individual, you would have found that the powers of his mind could only have been equalled by the virtues of his heart.

I now, with diffidence, ſubjoin an account of my own experiments.

Being acquainted by obſervation, and by the experience of others, with the Galvanic powers of black lead, I attempted to form a range of plumbago crucibles, into a Galvanic apparatus,

apparatus, which has the advantage of cheapneſs, and conſiderable power, and would no doubt anſwer very well, if a way could be diſcovered for preventing the moiſture from penetrating through the pots, though I have not been able to effect it by any mode of coating them: in caſe anybody ſhould be inclined to try this method, I have annexed a drawing of the manner in which the pots are connected, at the ſame time obſerving that the zinc pieces ought to be ſoldered to the braſs wire. If ever Galvaniſm can be applied to the arts, by procuring alkali in an economical proceſs, the above bids fairer to attain that end than any thing I have yet met with, as the pots are in a ſhort time covered with an efflореſcence of ſoda when common ſalt is uſed, which might be collected tolerably pure from their outſides.

In this range the zinc became the gaſeous, and the plumbago the oxidating ſide, which ſhows that it is as far beyond zinc, as zinc is beyond ſilver, in the arrangement of Galvanic oxidating power, though there is no reaſon to believe that the plumbago undergoes any change itſelf. When connected with a bent ſyphon, filled with diſtilled water, by either ſilver, braſs, or iron wires (even though the bend was filled with quickſilver), both ſides ſoon changed the reddened litmus paper to the purple, though the plumbago ſide reſtored it to be a perfect blue; ſhowing that a much greater quantity of alkali was formed on that ſide than on the other.

The only apparently electric phenomenon, independent of the ſhock, that I could obſerve by this range, was that the powder of the plumbago aſſumed the form of little ſtars on the ſurface of the water, which ſome people might be inclined to think proceeded from a ſtate of poſitive electricity.

Having lately diſcovered a mode of encreaſing prodigiouſly the powers of the pile, I have laid aſide the cumberſome and bulky apparatus of the range, and cannot recommend it for any thing but its cheapneſs, and the poſſibility of being ſatisfied at a moderate expence, by means of the larger ſized crucibles, of the proportion that bulk or ſurface bear to alternated numbers in Galvanic power.

I alſo annex a drawing of the box, in which I encloſe my pile, and which I can recommend for convenience and portability; the improvement that I have made in the pile is on the matter by which the pieces are put together, to which I was led by theory.

I took a pound of pipe clay, an ounce of plumbago powder, an ounce of black oxide of manganeſe, and two ounces of common ſalt, and made them into a paſte with water; in the uſe of clay I took advantage of the obſervations of Dr. Moyſe.

This mixture gave 126 pairs of ſilver and zinc of the half-crown ſize, a power beyond any thing I have yet heard deſcribed, and gave the ſpark moſt vividly, in a room where the ſun was ſhining, either from a bit of common coak, or the point of a needle; but even when two pieces of metal were held in the hand, though perfectly dry, and the wires touched by them, a ſhock was felt much greater than that procured from a very conſiderable number of pieces with the aſſiſtance of moiſture, ſet up in the common way. When the pile begins to weaken a little, if one of the wires is allowed to vibrate on the piece of metal

in one hand, and the other wire held by the other, the ſenſation of this dry ſhock brings a complete recollection of what is commonly called the ſleeping of a limb, viz. that prickling that is felt after the limb has been long in a conſtrained poſition, and when, I believe, medical people ſuppoſe there has been a preſſure on a nerve.

I am almoſt inclined to believe, that the natural ſenſation proceeds from the Galvanic fluid in the human ſyſtem attempting to reſtore its equilibrium by repeated ſhocks. A pile in this very ſtrong ſtate gives no ſigns of electricity by a very delicate electrometer (which I uſed to the excluſion of the modes of accumulating electricity, on which I think little dependence can be placed), and no effect could be excited, even though the pile was inſulated, and the ſhock taken through the metallic arm that ſupported the electrometer, by a perſon on an inſulating ſtool.

I alſo attempted to charge a Leyden vial in the way* mentioned by you, as uſed ſucceſsfully by Mr. Cruickſhanks, viz. connecting one end of the pile with the outſide, and the other with the inſide of the jar. It ſtrikes me that it is impoſſible a jar would be charged in that manner, as the two ſides, in that caſe, are connected by conducting ſubſtances, the very mode uſed for diſcharging a jar; if I have miſtaken your meaning, or that of Mr. Cruickſhanks, I beg you will correct me. I alſo connected both wires with the outſide, and both with the inſide, and even the ſilver wire of one pile, and the zinc wire of another, alternately with the internal and external ſides, without producing any effect.

I then attempted to charge the vial through the inſulated pile, connecting the wires by turns with the conductor of an electrical machine and the inſide of the jar. By comparing the change produced by a definite number of the revolutions of the machine, when attached and unattached to the pile, and when the ſilver and zinc wire were alternated, in the manner above deſcribed, no difference could be perceived either in the ſhock or ſize and length of the ſpark: alſo when the jar was placed between the conductor, and the inſulated pile, no difference could be perceived by taking the ſhock or the ſpark through the pile.

Theſe experiments were often repeated with the ſame reſults; and I think they ſhow clearly that the charge is neither accelerated nor retarded by the pretended *plus* and *minus* of the pile.

I have ſeveral times obſerved, and wiſh it may be attended to by others, that when women and men form a part of the Galvanic circuit, that the women feel almoſt the whole

* Your words are---" That Mr. Cruickſhanks, of Woolwich, has charged a large jar ſo as to give a " ſhock merely by placing the pile between its coatings." This I ſuppoſe was a private communication, as I have in vain looked over Mr. C.'s own papers for the fact.---C.

The communication was made in converſation. I think the arguments and inductions at p. 243-- 245 of our IVth volume remove many of the difficulties of theory, ariſing from the low intenſity of the power; but not this of the actual ſhock. I have charged talc as well as thin ſilk unequivocally; but expect ſoon to repeat the experiments with a higher power.---N.

 ſhock,

ſhock, when the men feel little or nothing; this even happens when only two perſons are in that ſituation, and with whatever ſide of the pile they may be connected, and when one woman ſtood between two men.

I believe we owe to the learned profeſſor of botany, in this univerſity, the obſervation, that plants are not conductors of the Galvanic influence; a moſt ſtriking diſtinction betwixt it and the electric fluid: I connected the pile, in its ſtrongeſt ſtate, with the ſtalk of a live and herbaceous plant, the ſtem of which was even moiſtened with ſalt water. The ſhock could not be procured from any part of the plant, however near to the wire, and one of its leaves applied to the wire, ſufficiently interrupted the Galvanic power*

The Galvanic ſpark procured from charcoal, has a very different appearance from the electric received in the ſame way; the former being of a red colour, and reſting upon the eye, whilſt the latter, notwithſtanding its ſuperior velocity, can be diſtinctly perceived, to have the blueiſh caſt of burning ſulphur.

The intention of this communication will be fully anſwered, next to preſerving a memorial of a perſon deſervedly dear to all his friends: if I ſhall be found to have added a drop to the ocean of ſcience, or to have facilitated in the ſmalleſt degree, the purſuits of others on the ſame ſubject,

I am, SIR,

with great reſpect,

A FRIEND TO THE EXPERIMENTAL RESEARCH OF TRUTH.

Figure 1ſt. A A repreſents the black lead crucibles, and as a range for Galvanic experiments. B B the connecting wires, to which pieces of zinc, as C, are attached by ſolder.

Figure 2d. is the pile box, of which D D D D D are the ſpaces, in which the piles ſtand. The ſhelf E E E, moveable eaſily for two thirds of the height of the box, by means of the rod F F, which paſſes through the top of the box, and may be faſtened by the flat wedge G; the rod is preferable to a ſcrew, being leſs hurt by moiſture.

Figure 3 is the ſhelf E, taken out to ſhew the manner in which it is formed.

Fig. 4, is one of the diviſions of the box which are expreſſed by H H, in Fig. 2, I being cut out for the ſhelf to run in.

N. B. A front piece ſhuts the box, by ſliding in a groove, on each ſide from the top downwards, which is ſloped away at the edges, and can be thruſt under the baſe of the box; which gives it an inclination backwards, that facilitates the building of the pile to any height. I have lately much improved the box, by lining it with oiled ſilk, with a view to prevent the wood from abſorbing moiſture.

* I have undoubted information that a portion of the aloe-plant, which was placed for a night in the connecting line of a pile of 36 half crowns and zincs was killed by its action, the other parts remaining unhurt.---N.

Third

II.

Third Communication on the Trains of Watches. By the Rev. W. PEARSON.

To Mr. NICHOLSON.

SIR,

WHEN I transmitted to you the two papers, contained in the third Volume of your Journal, respecting the philosophical uses and trains of watches, I had not an opportunity of satisfying myself with regard to one material consideration in the construction of those delicate machines; namely, whether or not the diameters of the wheels ought to be successively diminished from the center wheel to the end of the train, in proportion as the maintaining power becomes exhausted; *four* beats in the second were then fixed upon, for the new trains recommended, for no other reason but because this seemed a convenient number to be divided and subdivided without a remainder, and was also very nearly equal to what one of the watches, under examination at that time, was governed by: since, however, I came to reside near London, I have inquired of some eminent watchmakers what number of beats in a second, is deemed in practice most desirable for preserving a steady equable motion in the going of a watch, and the information I have received, particularly from Mr. Iles, of St. Martin's Court, is, that about *five* is found to be the best number of any: now as the number of beats of a watch in a second has been shown to depend upon what may be called the third, or last portion of a train, (in a watch which shews seconds) with the contrate wheel, its pinion, and the balance wheel; and as five beats in a second require higher numbers, in either one or both of these wheels, than four do, the inference is, that, allowing 15 to be the best practical number for a balance wheel, the contrate wheel must necessarily be larger than I have before given it, in order to produce the desired effect of five beats in each second.

Hence arises the necessity of my troubling you with a third communication on this subject, which, I am persuaded, you will readily lay before the public, not only because watches are in the hands of every reader, but also because you are aware, that they can be made to measure fractional parts of a second for as little expence, as they are generally constructed now that no attention is paid to the exact measure of a beat.

In order to make a watch beat exactly five times in each second, whilst 15 is the number of the balance wheel, the contrate wheel and its pinion must be to each other in the ratio of * 10 to 1; so that if 6 be the pinion, 60 will be the wheel; if 7 then 70; if 8, 80, &c. and as experience seems to point out the expediency of retaining 15 as the most

† When the beats of a watch are four per second, the ratio between the contrate wheel and its pinion is only as 8 to 1.

eligible

eligible number for the balance wheel, any one of the above pairs of wheels and pinions may be adopted, that the artiſt finds his other work will admit of:—what appears to me, on conſidering the ſubject in a practical point of view, as the moſt eligible train, out of the many that have been ſuggeſted, is $\frac{64}{8} \times \frac{60}{8} \times \frac{60 \times 15 \times 2}{6} \div 3600 = 5$, which may be otherwiſe, perhaps more intelligibly, expreſſed thus:

64 Center wheel
Pin. 8—60 Third wheel
Pin. 8—60 Contrate wheel
Pin. 6—15 Balance wheel
2 Palettes.

Beſides the advantage that this train, or one of a ſimilar value, has over thoſe before ſpecified, in point of producing a ſteady equable motion, it has the further recommendation of being capable of being counted *decimally* without any trouble; for when any number of beats are reduced into ſeconds by the diviſor 5, the remainder *doubled* will, in every inſtance, be ſo many tenths of a ſecond over, and will be an even or odd number, accordingly as the termination of counting by the beats coincides with a beat, or falls in the interval between two; in the latter of which caſes there will be, beſides the beats remaining after diviſion, an half beat to be alſo doubled: thus, for example, in 132 beats of a watch, which beats five times in a ſecond. there are $26\frac{2}{5}$ or 26,4 ſeconds, but in $132\frac{1}{2}$ there are $26\,\frac{2\frac{1}{2}}{5}$ or 26,5.

When your readers conſider what expence has been incurred in improving watches by the introduction of detached eſcapements, compenſation balances, &c. whilſt notwithſtanding a *ſecond* is the ſmalleſt portion of time they are hitherto intended to meaſure, they will, doubtleſs, ſee the propriety of making choice of ſuch numbers in the conſtruction of thoſe portable chronometers, as will not only tend to improve their rate of going, but will render them, without any additional expence, capable of ſubdividing the ſecond into exact *tenths*, and conſequently of becoming admirably ſubſervient to a great variety of philoſophical reſearches, in which a minute attention to time is indiſpenſably neceſſary.

In a watch conſtructed to beat five times in a ſecond, the main-ſpring muſt be ſomewhat ſtronger than in one which beats only four times, becauſe the contrate wheel, which is a *driver*, has its diameter enlarged; but what appears to me to be the greateſt practical objection to this conſtruction, is that which applies to the deſtructibility of the laſt pinion; by a careful examination of the works of an old watch, it will appear, that the pinions wear out before the wheels, and firſt of all that pinion which is placed on the arbor or axis of the balance wheel, by reaſon of the number of its rotations: on this account, particularly where the ratio between the contrate wheel and pinion in queſtion is ſo high as 10 to 1, the *temper* of the pinion ought to be well attended to, which is uſually left too *ſoft* by the

the workmen; partly, I ſuppoſe, becauſe the pivots are afterwards more expeditiouſly finiſhed, and partly becauſe a watch is thereby liable to be the ſooner unſerviceable.

There is, however, one recommendation more of a large contrate wheel, which I ſhall juſt mention before I conclude the ſubject: as the arbors of the contrate and balance wheels of watches of a common conſtruction, placed one horizontally, and the other perpendicularly, they are neceſſarily laid at one ſide of each other, ſo as juſt to eſcape touching as they croſs, by reaſon of which poſition an obliquity is occaſioned in the direction of their action, which is the greater the ſmaller the contrate wheel is, and *vice verſâ*; and which frequently occaſions the ſtopping of the watch: an enlarged contrate wheel will conſequently greatly diminiſh this cauſe of imperfection, and will therefore conſtitute an additional improvement.

I am, SIR,

Your's reſpectfully,

W. PEARSON.

Parſon's Green, April 13, 1801.

III.

A Method of meaſuring a Ship's Way at Sea, with Obſervations on other Branches of the Art of Navigation. By JOHN COOKE, *Eſq. M. R. I. A. Communicated by the Author.*

LET a tube with a few inches of one end bended to a right angle, be affixed to a ſhip, in ſuch a manner that the longer part may enter through the ſhip's bottom below the water line, and be faſtened in a poſition parallel to the maſt, and ſo that the recurved end ſhall be immerſed in the water in a poſition parallel to the keel, with its orifice pointed towards the ſhip's head. Let the upright part of this tube be furniſhed with a ſmall ſpouting aperture within the ſhip, at the level of the external water; then ſuppoſe it to move ſteadily through ſmooth water in the direction of its length, the impulſe of the ſtream on this orifice will be as the ſquare of the ſhip's velocity (ſee note A). And ſince the column of water thrown up in the erect tube by this force muſt be in equilibrio with it, the perpendicular height of this column of water will be as the ſquare of the ſhip's velocity alſo; in which caſe the water will flow from the aperture into the ſhip, while the veſſel moves forward, and the quantity diſcharged will be in a ſubduplicate ratio of the perpendicular altitude of the column in the erect tube; that is in the ſimple ratio of the ſhip's velocity. If therefore the quantity diſcharged through this aperture in paſſing over a mile be obſerved, the actual diſtance run at any time may be found, if the water ejected be preſerved and meaſured (ſee note B).

But the pitching of a veſſel at ſea, and the motion of lee way, would cauſe the ſtream to fall obliquely on the orifice, and conſequently affect the preſſure and alter the ratio which

which exiſts between it and the ſhip's motion in direct progreſs. To prevent this, a ſhort tube is to be fitted to the extremity of the immerſed tube, in ſuch a manner as to be capable of moving freely in a vertical, in an horizontal, or in any intermediate arch (which may be effected by a joint conſiſting of a portion of a ſpherical ſhell containing a ſimilar ſhell) to be furniſhed with vanes extending behind the joint, which are to be placed in ſuch a manner, that the ſtream acting on them may point the orifice ſo as to meet it perpendicularly; but a ſhip at ſea is often deflected from her path into a curvilinear track by the figure of the waves; and ſince the ſtream is cauſed by the relative motion of this water to the ſhip's motion, which may always be conſidered as in the direction of the keel, it muſt meet the orifice of the inſtrument in the direction of the keel, (ſee note H) that is perpendicularly, and therefore the water delivered through the ſpouting aperture muſt repreſent the undulating curve through which the ſhip moves, inſtead of its abſciſs, or true diſtance, (ſee note C) which might ſometimes differ conſiderably from it; for which reaſon the following method of correcting it becomes neceſſary. Let a barometer made narrow in the middle to check the ſudden motions of the mercury, be placed againſt a bulk-head, or tranſverſe partition of the ſhip, and ſo loaded and hung that when the ſhip heels it may ſwing in an arch deſcribed on the ſurface of this partition, ſo as not to partake of this lateral inclination (which is not difficult to accompliſh, the motion of heeling being neither very ſudden nor fluctuating) but in ſuch a manner as to receive the intire effect of the pitching motion which it is intended to repreſent; then if the curve which the ſhip deſcribes be ſuppoſed to be divided into indefinitely ſmall equal right lines, any one of them will be to the portion of horizontal diſtance which it overhangs, as radius to the coſine of the angle which it forms with the horizon; but the length of the column of mercury in the inclined poſition of the barometer, is to its length in an erect poſition as radius to the coſine of the ſame angle; therefore the length of the column of mercury at any time will be to its perpendicular height, as the rectilinear portion of the path which the ſhip ſhall be then deſcribing to true diſtance; and ſince theſe rectilinear portions are equal, and paſſed through in equal times, the mean of them obtained by the narrowed tube will repreſent them all, therefore the length of the column of mercury marked on the ſcale of the barometer will always be to its perpendicular height as the ſum of theſe rectilinear ſpaces; that is, as the curved path of the ſhip repreſented by the water diſcharged by the inſtrument, is to the ſum of the horizontal ſpaces overhung by them; that is the true horizontal diſtance, from which data the inſtrumental diſtance may be eaſily corrected at any time. But it is to be obſerved, that the height of the mercury in a perpendicular poſition is variable, on account of the difference in the weight of the atmoſphere at different times; therefore when it is required, the barometer muſt be taken from its place, and held carefully by the hand for a few minutes in an upright poſition, until the mercury ceaſes to deſcend, when it will ſhew the true perpendicular height neceſſary to this experiment, and at the ſame time the barometrical ſtate of the atmoſphere (ſo uſeful at ſea) more accurately than any ſuſpended barometer.

The agitations of the fea affect alfo that part of the inftrument which lies within the fhip, and it remains to be confidered what confequences arife therefrom. Heeling, or an inclination of the veffel to one fide, and pitching, or the librations of it fore and aft recline the upright tube, and thereby lengthen the column of water contained; but fince the difcharge of water is affected by *perpendicular* height only, this is but an apparent caufe of error; the fame preffure at the immerfed orifice will elevate the water to the fame *perpendicular* height whatever the length of the column may be, and therefore the velocity of the effluent water will be the fame with the fame degree of preffure whether the tube be erect or inclined; plunging, or the afcending and defcending motion, occafioned by the waves which alternately elevate and deprefs a veffel in the fea, by altering the weight on the orifice increafe or diminifh the efflux of water from the tube, to remedy which a part of the upright tube below the water line is made narrow, in the manner hereinafter defcribed, whereby the afcent and defcent of the contained water will be rendered gradual, fo that its altitude may become the mean of feveral alternations of preffure; and fince the waves fucceed each other at equal intervals, and of equal dimenfions for a confiderable time together, this mean height will be fufficiently fteady and exact; (fee note D) and though this motion and pitching may feem to difturb the operation of the fpouting aperture, by raifing and finking it above or below the plane of the water line, it occafions equal and oppofite errors thereby, which remedy each other, and narrowing the tube is really ufeful only to procure a more equable flow from the aperture; but heeling, which is a more durable pofition, has a tendency to raife the fpouting orifice above the plane of floatation; however, this elevation is fo fmall, that the error arifing from it is infenfible.

Some mechanical difficulties prefent themfelves in applying this inftrument to a fhip, and in adapting it to the purpofes of navigation; firft it may be thought unfafe to make a hole in the fhip's bottom to receive the tube, but the experiment has been tried to fupply fire engines with water on fhip-board, and no inconvenience arofe from the practice when the receiving pipe was clofely fitted to the perforation. It might alfo be paffed, if thought preferable, through a hole above the water line, and the neceffary curvature given without the fhip.

Secondly, it appears from calculation, that the water in the erect tube would fometimes afcend fo high, as to require a tube inconveniently long to contain it; but if it be cut off any where above the fpouting aperture, and if the upper orifice be clofely ftopped, it will anfwer the purpofe as well as before, becaufe the preffure which the ftopper receives, is that which would fupport the column of water cut off, and therefore the preffure which this ftopper gives, is that which would arife from the weight of the column cut off. Hence the preffure of the water within the tube continuing the fame, the rate of difcharge will remain unaltered by fhortening it.

Thirdly, it is difficult to afcertain the true mean level of the fea when the fhip is in motion, which may often be requifite; in this cafe, if a flat piece of metal be fixed on the

top

top of a pole, and held out of the ſhip ſo as to defend the external orifice from the percuſſion of the water, the contained water will ſink to the true mean level, and the poſition of the ſpouting aperture, if fixed in a ſliding plate, may be adjuſted thereby *

Fourthly, if dirt or weeds get into the inſtrument, it may be cleared by forcing out the contained water by means of the plunger hereafter deſcribed; or if this plunger and the ſtopper be drawn out, the water will ſpout up forcibly in ſailing at a moderate rate, and thus remove the obſtruction.

The advantages expected from this inſtrument are, that it propoſes a *perpetual* meaſure, which adapts itſelf to every alteration of the ſhip's motion, and therefore corrects that error ariſing from the log, the uſe of which ſuppoſes the ſhip's motion to be uniform during the invervals of experiment, which is highly improbable.

And ſecondly, that the inſtrument being ſunk to a conſiderable depth, will not be affected by ſuperficial currents occaſioned by wind, to which the log is expoſed.

The cauſes of error which affect this inſtrument, are the ſame which attend the methods of navigation in general uſe.

The error of yawing is that which ariſes from unſteady ſteering, and will deflect the orifice from the true poſition.

If it be admitted, that the generality of ſeamen ſteer ſo as to confine the yaws within certain limits, the true line of motion may be conſidered as a ſerpentine line, which has a fixed ratio to the right line which joins its extremities in all caſes; but if the quantity of diſcharged water which repreſents a mile, be found from a mean of ſeveral experiments made at ſea (which experiments are of courſe affected by yawing) it follows that the ſtandard meaſure thus eſtabliſhed needs no correction, the effect of yawing being proportionate, and being allowed for in aſcertaining the ſtandard quantity.

The error of *lee-way* may be corrected, if a metal rod be extended along the axis of the upright tube, and paſſing through its bottom into the water below it, it ſhould be capable of being turned round in any direction by the ſtream acting on a vane like that of a weathercock, which is to be annexed to it below the tube; on the top of the tube is to be placed a circular plate engraved with the points of the compaſs, through the center of which this rod is to paſs, and to bear an index round it, which will point out the angle of lee way on the compaſs plate. Since the ſtream below will keep the vane in the ſhip's true line of motion, if this rod be furniſhed with a circular protuberance, nearly filling the tube below the ſpouting aperture, the ſudden current of the water within it will be thereby checked and rendered gradual, as was propoſed before; and if this rod be divided into two parts juſt below the protuberance, and if the termination of the lower part be ſquare, and if the bottom of the protuberance contain a ſquare ſocket, theſe parts may be occaſionally

* Or otherwiſe, if the external aperture of the tube itſelf, or of any other tube, could be placed at right angles to the ſhip's motion, the water would during that time ſtand at and ſhew the external level.---N.

 ſeparated

ſeparated to plunge and clear the tube when neceſſary (ſee note G). The variation of the needle from a polar direction is another courſe of error in navigation, but the correction by azimuths and amplitudes affords a perfect remedy for it, and when the quantity of variation is diſcovered, the dead reckoning may be corrected thereby by well known means; alſo the variation may be found in a more ſimple manner at all times of the day, by annexing any of the univerſal ſun-dials to the compaſs box, in ſuch a manner that the meridian of the dial may be parallel to its ſide, and by adjuſting it to the latitude of the place; for if ſuch a dial be turned until the ſhadow points to the hour found by a lately corrected watch, its meridian, and the ſide of the box to which it is attached, muſt be in a true north and ſouth direction, and the deviation of the north point of the card from this meridian will be the angle of variation. (See note E)

Currents are the ſources of the greateſt errors to which navigation is liable; the method of finding their ſet and drift by the courſe and rate at which a ſunken body ſeparates from a drifting boat is erroneous, being grounded on a ſuppoſition, that this body meets with ſtill water, whereas it is believed, that ſuch a maſs of water moves in a current together, that our experiments only penetrate its ſurface; however, this method ſeems to be ſufficient to determine the exiſtence of a current, and the rhumb on which it flows, becauſe every ſtratum of moving water runs with a different degree of velocity, and this will be the caſe even if the ſunken body ſhould deſcend into ſtill water, or into a contrary current (and ſince interſecting currents have never been obſerved, we may conclude, that this experiment will, in all caſes, prove the exiſtence of a current, and ſhew the line of its motion, though it remain uncertain to which extremity of that line it may flow.

If the preceding methods of determining courſe and diſtance can be relied on, the error ariſing from a current which does not flow in a parallel of latitude may be aſcertained by attributing the error of dead reckoning to the line of the currents motion, and then by adding or ſubtracting, as the caſe may be, ſuch a ſupplementary diſtance in the direction of that line, as will make the dead reckoning correſpond with the latitude by obſervation, which will give the true place of the ſhip; and if the currents be regular and periodical, as is ſuppoſed, numerous experiments made in this way at various times and places, might afford materials for the conſtruction of tables, or hydrographic charts, pointing out the drift ſet and periods of theſe currents, which would enable the navigator to make the neceſſary allowance for their effect in all caſes.

Alſo if the true direction of the wind can be aſcertained in a current, the lines of its motion being known, the ſet and drift may alſo be known, becauſe if the ſhip lie to, the apparent wind will be a compoſition of the true wind, and of the wind ariſing from the relative motion of the air produced by the running water (ſee note F), which, in moſt caſes, bears a very ſenſible proportion to the true wind (ſee Encyclopædia Britt. Vol. XVII. p. 204); therefore if the veſſel ſail along the current line, varying the rate of her motion until the wind vane lie exactly in the direction of the true wind, there will be no current

wind, and, conſequently, no current, and the veſſel being then ſtationary with regard to the earth, the rate and direction to the ſtream will be the ſet and drift of the current. However, ſince no adequate method is known to determine the true direction of the wind in a current in all caſes, this obſervation would be uſeleſs; but that there are ſome grounds to ſuſpect that within the tropics the apparent deviations from the trade winds ariſe from the deflecting force of current winds. If this be ſo, and if the general motion of the trade wind be aſſumed as the true direction of the wind, this rule will ſhew the ſet and drift of the currents in a great part of the ocean.

NOTES.

A. This law has been denied in caſes where a diminution of preſſure on the back part of the moving body ariſes from its motion, but the altitude of the water in this tube is affected only by the *abſolute*, and not the relative, impulſe of the impinging current (which is apparent from the form of it), and Mr. Vince's late experiments tend to prove the truth of the theory in ſuch caſes.

B. If the ſpouting aperture be very ſmall, the theory is true, but when the aperture is large, it is not ſo certain for this reaſon; and alſo becauſe it will be more convenient to receive a ſmall than a large quantity of water into the ſhip, the hole through which the water iſſues ſhould be very ſmall, with even thin edges. See Vince's Hydroſt. p. 54.

C. It may not be uſeleſs to obſerve, that if the ſpace repreſented by the inſtrument in paſſing over any certain number of ſuch waves be obſerved and divided by the number of theſe waves, it will give what is called the breadth of the wave; and ſince the celerity of a wave is demonſtrated to be as the ſquare root of its breadth, the rate at which the wave moves may be found by a table at any time by the help of this inſtrument.

D. Nairne's marine barometer is formed on this plan, and alſo the tide guage.

E. It may be curious, if not uſeful, to obſerve, that it is poſſible to ſteer by the ſun alone without the aid of magnetiſm.

If there be a large watch, whoſe hand performs one revolution in twenty-four hours, with ſuitable graduation on the dial plate; this dial plate, with a perpendicular ſtyle in the centre, when reclined according to the latitude, becomes a ſun dial; and when the ſhadow of the ſtyle coincides with the hand of the watch, the ſtyle muſt hang over the true meridian: therefore if this meridian be ſet to the ſhip's courſe by a motionleſs compaſs card, and if the helm be ſo managed as to keep its ſhadow conſtantly on the hand of the watch, the true poſition of the ſhip will be preſerved until the time of the watch or the elevation of the ſtyle require correction. It is poſſible to effect the ſame thing by a ſtar, by having a dial plate formed of ſome reflecting ſubſtance.

In climates where the ſky is ſeldom obſcured, ſuch an inſtrument would afford a ſteady guide to the mariner, not being liable to the errors of variation, aurora borealis, or to the deflections of libration, which diſturb the ſuſpended card very conſiderably.

F. It is a fact, well known at ſea, that the apparent wind is compoſed of the true wind, and of the wind occaſioned by the ſhip's motion; therefore it is that a ſhip ſailing on different tacks, or with a different degree of velocity in the ſame line, will have the wind in a different point, becauſe one of the component forces is altered in poſition in the former, and in intenſity in the latter caſe. What we experience in riding through a ſhower of rain is a familiar example of this fact; while the horſe's motion is uniform, the direction of the drops appears to be conſtant, but if it be encreaſed, the direction of the drops will be more in the rider's face; if it be diminiſhed, it will be more behind him; and if his road be changed to the right or left, the ſhower will apparently alter to a point more oppoſite to his motion than the point it ſeemed to be in before.

G. Should it be conſidered adviſeable to diſencumber this inſtrument by ſeparating the lee-way machinery from it, it may be annexed to the compaſs box in the following manner:

A metal rod is to be extended down the ſtern poſt, and ſo hung as to be turned eaſily on its axis by a vane at the bottom, which is to be acted on by the ſtream. To the upper part of this rod, juſt below the level of the deck, a croſs bar of a few inches long is to be annexed. The compaſs box muſt reſt on an upright foot, extending a little below the deck, and ſupported by a ſocket, in which it is to turn freely. Juſt below the deck a croſs bar is to be annexed to this foot equal to that on the ſtern poſt rod, and the extremities of the two croſs bars are to be connected by two ſlender equal bars moveable on pivots, ſo that the four bars may form rhomboides, in conſequence of which the oppoſite ſides muſt be always parallel, and the motion of the foot will be the ſame as that of the ſtern poſt rod: therefore the vertical ſection of the compaſs box, which paſſes through the centre and ſteering mark of the box, will be parallel to the vane; and ſince the ſtream will point the vane exactly to the ſhip's true line of motion, it follows, that the ſteering mark on the compaſs box will be pointed to the true line of motion alſo, and the angle contained between this mark and the north point of the card will conſequently be the true magnetic courſe clear of the error of lee-way.

IV.—*On*

IV.

On the Escapements of Time Pieces. By Mr. THOMAS REID.

To MR. NICHOLSON.

SIR,

Edinburgh, April 6, 1801.

BEING lately favoured (by a friend, and one of the most eminent philosophers of this place) with a reading of your useful Journal, chiefly with a view that I might see the account given of Mr. Prior's escapement, in which I was somewhat interested, having made one of the same kind in the year 1793, and which watch is at present in the hands of Mr. Howells, No. 15, White Hart Row, Kennington Lane, Surry.

The originality of invention that seems to attach to Mr. Prior, I lay no claim to; as by a note it appears that he had not seen the escapement of Mr. Mudge, and I may add, nor perhaps that of Lepaute's. Although I had not seen Mr. Mudge's escapement, some hints respecting it were given me in an obscure manner, but yet so as by them I could comprehend it. The escapement of Lepaute's I was perfectly acquainted with; and it was in attempting to combine his with Mudge's that led me to make the one now in question.

In mine, the time of rest of the teeth, and the flanch for the impulse, is on the inside of the pallets; and in that of Mr. Prior's they are on the outside.

There is a very material circumstance which Mr. Prior has not perceived. His pins are arranged in the same line of circle on both sides of the wheel. This will make a considerable difference, especially if the pins should be left pretty thick, which will be the same as if the pallets were impelled by wheels of unequal diameters, and they will consequently receive unequal impulses.

That pallet which moves inwards, receives its impulse from the outward part of the pins. The pins ought to be so disposed as to give equal impulses. For that purpose, let a very fine circle be traced on the side of the wheel, let the holes for the pins, that are to act on the pallet outwards, be drilled inside of the circle, and so as that the edge of the holes shall coincide with it: those pins for the inward pallet must be drilled without the circle, and to have the edge of their holes also to coincide. In this case, the impulse given to the pallets will be equal; but then it may be said that the rest is not equal, this last is so trifling, that of evils it is the lesser.

In this case of an escapement wheel *with pins* on each side, the impulses and times of rest are brought nearer to an equality than in Mudge's, yet his teeth have some advantages which the pins want.

The roller attached to the verge, and serving as a pallet, is not new. Le Roy, Berthoud, and others, had used such, nor do I see much benefit to be derived from it. The watchmakers of Chaux de Fonds, Locle, and of Geneva, think so little of this, that they use simply

simply a piece of brass for this pallet. You must know that *they* have long ago and often made up watches and clocks with Mudge's escapement to them; probably from Berthond's carrying it to Paris with him; Mr. Mudge having shewed him the escapement when he was in London, in 1766.

I see you wish to be informed, Who was the inventor of the compensation watch at present in use? I can inform you this, it was the invention of Le Roy, as you may see in a book, entitled, "Voyage fait par ordre du Roi in 1768, pour éprover les montres Marines, inventées par M. Le Roy, par M. Cassini, Fils."

The invention of the horizontal escapement you have attributed to Debaufre. The escapement of Debaufre has not the smallest affinity to the horizontal one, nor can it give any idea of it, or tend in the least to lead to it.

Mr. Tompion certainly gave the idea of the horizontal escapement, in 1695; to be sure it was not in his hands just what it now is, but in the state such as he left it, was Mr. Graham (who, I believe, was his apprentice) afterwards led to improve it, and ultimately to complete it, prior sometime to 1730.

Debaufre's escapement had two balance wheels on the same axis, and was a contrate wheel watch. It was the first watch that perhaps was ever jewelled in the verge holes. In his attempting to improve the escapement, he formed a connection with a M. Facio, a native of Geneva, who, in 1700, had invented the piercing of rubies and of watch jewelling, and not being well received with his invention at Paris, Facio came over to London; and set to work with Debaufre at the said escapement: its pallet was of diamond.

There are watch-makers at present in London who may remember a Debaufre, a watch-jeweller, and one of the earliest of that business, who died about 1774, or thereabouts: he was old, and perhaps might be the son of the former. The introduction of watch jewelling* into Britain, you see, arose from an accidental circumstance; and it is strange that to this day it is hardly known in France, unless of late some one may have left this conntry, unfortunately by want or by opinion, and carried the art there. Should these lucubrations be favourably received, I may have something more to communicate to you afterwards.

I am SIR,

With much esteem,

Your most humble servant,

THOMAS REID.

Watch-maker, Edinburgh.

* Watch jewelling has contributed not a little to the celebrity of our watches.

Pray

Pray who first applied the duplex escapement, in London, to watches? It is the invention of a Dutertre early in the eighteenth century *. How long is it since watches with the virgule escapement were seen? and who was he who applied it first?

See Regle Artificielle du Temps, par M. Sully, respecting Debaufre's escapement; it is but rather treated of there obscurely, yet I am quite well aware of its construction. In the spelling of escapement I have used an ellision for the *e*; however, you may adopt which way you think best. We use the word *'scapement* in common conversation.

I wonder much at those gentlemen of l'Academie Royale des Sciences so grossly mistaking or misrepresenting what Sully has written; nothing so clear to me as the account Sully gives of Tompion's escapement being the foundation of the horizontal one. Tompion gave it up for a bad job, and this might be one reason why it was so late before it was completed under the hands of Graham. Those gentlemen of the academy are equally mistaken in their ideas of improvement given by P. Le Roy to Debaufre's escapement, which they call the horizontal. What P. Le Roy did was, he took one of the wheels, and substituted another pallet; but nothing here all the while but is most distant from the horizontal one. It is true that Sir Isaac Newton had one of Debaufre's watches, which he showed to Sully in 1704. I do not know who this Gourdain was, who is also mentioned. It must be Gaudron that they mean. He was a man of genius, and watch-maker to the Duke of Orleans, during the Regency †.

* N. B. Enderlin takes the merit of being the inventor as well as Debaufre, and P. Le Roy claims that of Dutertre. See Lepaute, Thiout, &c.

† Gourdain was a watch-maker probably of Paris. His communications were made to the academy in 1742.

The commission of the academy, in their report on M. Gourdain's improvement of Baufré's escapement (Recueil des Machines, VII. 137), describe this last as follow: I translate verbally.

" To remedy this inconvenience (the acceleration in the crown wheel and pallets, when the first mover is augmented) " the Sieur Baufré, a French watch-maker, settled in London, imagined, in 1704, the " escapement called dead beat (*qu'on nomme à repos*). He suppressed the two pallets and the crown wheel, " and fixed on the arbor of the balance a kind of cylinder, rather thick, interrupted in one of its halves, " and which, instead of its being terminated in its section by the plane which would pass through its axis, " is on the contrary (terminated) by two inclined planes, one on each side of the axis.

" Opposite these two inclined planes are two wheels fixed parallel to each other on the same axis, placed " so that the teeth of the one are opposite the interval formed by the teeth of the other. By this means, as " soon as one tooth has slided along one of the inclined planes and impelled the balance, the other tooth " reposes *on the superior base of the cylinder* which carries the inclined plane; so that the balance can obey " the whole impulse it receives, and run through arcs as large as are necessary, which gives a facility of " regulating the going of the watch, and taking advantage of all the accuracy which the spiral spring at- " tached to it is capable of giving."

The reader will perceive how I myself, and not the academicians, have been mistaken in my note at page 53 of the second volume of this Journal, in concluding this to be the horizontal escapement, to which, indeed, the description would apply if we were to overlook the words here put in Italic, as I did, and infer that the places of repose were on the curved surfaces of the cylinder.

Gourdain's escapement is precisely enough described in the note last quoted.---N.

What has given credit to Pinchbeck as the inventor of a watch thermometer, muſt have been the circumſtance of his purchaſing from Berthoud, when in London, 1766, for his preſent Majeſty, a watch with center ſeconds, the common Verge Eſcapement, and having a thermometer, or compenſation piece, which Berthoud ſays, was the firſt pocket watch made with ſuch a thing. Kendal copied this compenſation work in ſome watches that he made afterwards.

Sully wrote his Regle Artificielle in 1717, and J. Le Roy gave an edition of it in 1737. He was an Engliſh watch maker; what time he went to France I know not, but he was a great genius, much eſteemed there, and had the merit of putting the buſineſs in a better train, reared up workmen, and gave them quite new methods. Julien Le Roy thought highly of him, and was himſelf a man of very uncommon ability.

Excuſe this deſultory manner, a little trouble may make it more connected.

T. R.

V.

Inquiries relative to the Laws of Affinity; by CIT. BERTHOLLET, *Member of the National Inſtitute of Arts and Sciences* *.

(Concluded from page 21.)

ARTICLE X.

On the Determination of the Elective Affinities.

1. TO determine the elective affinity of two ſubſtances for a third, according to the notion which we ought to conceive of it, conſiſts in aſcertaining in what proportion the third ſubſtance divides its action between the two firſt, and to what degree of ſaturation each of theſe muſt arrive when their forces are equally oppoſed. The relative affinity will be in proportion to the degree of ſaturation they would attain, on account of the quantity which ſhall have acted; ſo that if the quantities were equal, the comparative degrees of ſaturation would afford the meaſure of the reſpective affinities.

2. When I ſpeak of the ſaturation of a ſubſtance, I do not mean that abſolute ſaturation in which all reciprocal action would ceaſe, but a degree of ſaturation which is eaſily diſtinguiſhed, and is common to all combinations. It is that of neutraliſation, in which none of the properties of the conſtituent parts predominate. The term of cryſtalliſation of ſalts, does not always coincide with that of neutraliſation; for example, with reſpect to the carbonates of alkali, which ſtill continue to afford indications of alkalinity, and with the acidulous tartrite of pot-aſh, which on the contrary retains an exceſs of acid. Nevertheleſs, we may take the laſt combination at the term in which it is neuter, becauſe it ſtill poſſeſſes the

* Tranſlated from the Annales de Chimie, XXXVII, 151.

the property of cryſtalliſing; it is this tartrite alſo which is neceſſarily obtained, when an exceſs of the baſe is preſent in the experiment; but when an exceſs of acid is required, the degree of ſaturation in which the acidulous tartrite exiſts, may be determined by the quantity of pot-aſh which is neceſſary to effect its neutraliſation.

3. One conſideration which is immediately ſeen to deſerve much attention, is, that on comparing the affinities, it is neceſſary to employ in all the experiments, the ſame proportions of all the ſubſtances which might be ſucceſſively put in oppoſition; becauſe, if the proportions vary, the reſult of the action will be no longer the ſame, nor can the affinity be repreſented by the ſame number. I ſhall proceed to illuſtrate this obſervation by an example:

Let the number 100 repreſent the pot-aſh, which is to be ſaturated by 100 parts of ſulphuric acid; I oppoſe to it 100 parts of ſoda; I ſuppoſe that after the action the pot-aſh will be found to have abſorbed 60 parts of the acid, and the ſoda 40; I ſhall then conclude, that the affinities of thoſe two baſes for the ſulphuric acid are in the proportion of 60 to 40. But 40 parts of pot-aſh remain uncombined; theſe ſtill continue to act, and contribute by their action to the diviſion of the acid; ſo that if this quantity were to be varied, the reſult would no longer be the ſame. If inſtead of 100 parts of pot-aſh and 100 parts of ſoda, I take only 80 parts of each, the uncombined remainder will afford 20 parts of pot-aſh, and another quantity of ſoda, ſo that the forces exerted by thoſe two parts, are no longer in the above-mentioned proportion; whence it appears that the two ſaturations cannot be in the proportion of 60 to 40.

4. But in order to aſcertain the degree of ſaturation acquired by each of theſe ſubſtances, it is neceſſary to effect a ſeparation; and this can only be obtained by elaſticity, by cryſtalliſation, by precipitation, or by the action of a ſolvent. But we have ſeen that theſe different means ought to be conſidered as foreign powers which alter the reſults, and determine the combinations that are formed, without our being able to meaſure their effect, ſo as to diſengage that of the elective affinity; and conſequently, that thoſe ſeparations which, in the articles I, II, III, have only been conſidered as the proportional effect of elective affinity, are really the effect of ſeveral joint forces, as is proved by the obſervations ſubſequent to thoſe articles.

When it is neceſſary, for inſtance, to aſcertain the quantity of ſulphate of pot-aſh and ſoda that might be formed, the force of coheſion not differing conſiderably in theſe two ſulphates, it is probable that it would not effect any great change in the proportion of the two ſalts which would cryſtalliſe; but it would be neceſſary to ſeparate the exceſs of alkali by alcohol, in order to obtain the whole cryſtalliſation. Now, the action of the alcohol being unequal upon pot-aſh and ſoda, would again produce a change. To theſe conſiderations we may add, that a change of proportions would not only cauſe the force of the ſoda and the pot-aſh (No. 1) to vary, but alſo that of the alcohol, without mentioning the affinity of the water, which ſerves as the ſolvent.

But the changes would be much more conſiderable, if barytes were to ſerve as a compariſon with either pot-aſh or ſoda; the force of coheſion poſſeſſed by the ſulphate of barytes would then operate in ſuch a manner, that it would leave the alkali only a very ſmall quantity of ſulphuric acid, which would be much nearer to a meaſure of the force of coheſion of the ſulphate of barytes, compared with that of the ſolvent, than of the affinity of the barytes compared with that of the alkali.

This is ſo true, that if, in order to compare the affinity of the acids for barytes, we were to begin by treating barytes with an exceſs of ſulphuric acid, it would be almoſt intirely precipitated, unleſs the acid were extremely concentrated; we could not diſtinguiſh the combined portion from that which is not, and aſſert, that barytes has a greater affinity for the former part than for the latter. This however is really ſtated, when it is aſſirmed, that the ſulphuric acid has a greater affinity for barytes than any other acid, becauſe a ſulphate of barytes is formed by precipitation. That effect is attributed to elective affinity, which depends more particularly upon the force of coheſion peculiar to the ſulphate of barytes.

It is therefore manifeſt that the elective affinity of any two ſubſtances, with regard to a third, cannot be determined by a direct experiment, even when the trial is made with two ſubſtances in a liquid ſtate, and which can be neutraliſed by ſaturation; ſince it is neceſſary to introduce foreign powers in order to aſcertain the ſaturation.

5. It has been ſeen in articles II and III, that the affinity of a ſubſtance may be compenſated for by its quantity.

From this conſideration, it would appear to be ſufficient if we were to diſcover the capacities of ſaturation which different baſes have for an acid, or different acids for a baſe, in order to aſcertain the proportion of their relative affinity: for this ſhould ſeem to be in the inverſe proportion of the quantities neceſſary for producing the ſame degree of ſaturation.

This conſequence is erroneous, when we wiſh to apply it to the elective affinities, becauſe as ſoon as two ſubſtances are preſented in order to combine with a third, new forces are eſtabliſhed, which not only determine other reſults, but even change the conſtitution of thoſe ſubſtances.

I compare the ſulphuric and carbonic acids. It is certain that if I bring a quantity of pot-aſh to a term of neutraliſation by the carbonic acid, it exerts a force as great as the quantity of ſulphuric acid which would be neceſſary to produce the ſame effect; and yet if I pour the ſulphuric acid upon the firſt mentioned combination, the whole of the carbonic acid will be diſengaged; becauſe, being no longer retained by an equal force, it again aſſumes the elaſtic ſtate; and even if it be abſorbed and retained by a ſufficient quantity of water, it will no longer be in the ſame ſtate of compreſſion; it will not have the ſame conſtitution; it will no longer be the ſame ſubſtance relative to chemical action. And laſtly, we muſt apply the obſervations made in the preceding numbers to the action of ſubſtances unequally ſaturated.

The compariſon between the capacities of ſaturation, though it may lead to important conſiderations, cannot therefore be applied to the determination of elective affinities.

ARTICLE

ARTICLE XI.

On some Errors which proceed from a false Notion of Elective Affinity.

1. I ſhall here diſcuſs ſeveral opinions which have been adopted relative to elective affinity; I ſhall ſhew how unfounded they are, and ſhall oppoſe to them the application of the principles laid down in the preceding articles:

Baumé has obſerved, that when the ſulphate of pot-aſh is diſſolved by means of heat, in an equal weight of nitric acid, cryſtals of the nitrate of pot-aſh are obtained by cooling: he attributes this decompoſition of the ſulphate of pot-aſh to reciprocal affinities which produce oppoſite combinations, but does not determine the cauſe of this contrary effect.

2. The explanation of this remarkable fact has been contradicted by Bergman. He obſerves, that there are ſome ſalts which have a tendency for an exceſs of acid, ſuch for inſtance as the acidulous tartrite of pot-aſh. He conceives that when theſe ſalts exiſt in a ſtate of neutraliſation, it is neceſſary to conſider their baſe as divided into two parts; one of which is particularly affected by the whole action of the acid, in order to form the acidulous ſalt, while the other part only tends to ſatisfy that portion of acidity which remains in the acidulous ſalt. This part of the baſe is therefore only retained by a feeble acidity, and may be carried off by an acid very inferior to that which enters into the firſt combination. Thus the acetous acid may carry off that part of the pot-aſh which, in the tartrite of pot-aſh, is ſuperfluous to the combination which conſtitutes the acidulous tartrite of pot-aſh, though this acid has an affinity much weaker than that of the tartareous acid itſelf.

Now, the ſulphate af pot-aſh is among the number of the ſalts which have a tendency to form an acidulous ſalt. Nearly two thirds of its baſe enter into this combination, and it is this part alone that is ſubject to the whole affinity of the ſulphuric acid. The remaining third may be ſeparated by an acid of an affinity inferior to that of the ſulphuric acid, ſuch as the nitric acid, the muriatic acid, and the tartareous acid. When the decompoſition is carried to this point, it ceaſes, whatever may be the quantity of the acid oppoſed: and if the quantity be not too great to prevent the cryſtalliſation, or if the exceſs be expelled by heat, an acidulous ſulphate is obtained by ſolution and evaporation, which forms permanent cryſtals in the air.

3. How could the illuſtrious Bergman have deviated from the path traced out for him by obſervation? It is proved even by his own experiments, that the acid which is ſuperabundant to the acidulous ſulphate of pot-aſh exerts its affinity, that it exiſts in combination, and acts in proportion to its quantity. For he obſerves, that if ſulphuric acid be added to the acidulous ſulphate of pot-aſh, this ſalt is diſſolved, and loſes its property of cryſtalliſing; that this exceſs of acid is expelled with difficulty, even by diſtillation in a retort; and in order to obtain this effect, it is neceſſary to fuſe the ſaline combination in a crucible, or to employ ſeveral times the repeated action of very pure alcohol.

4. The limit given by Bergman to the action of the acids upon the acidulous ſulphate is therefore imaginary. This ſulphate acts like all the ſalts which are capable of reſiſting, to

a certain

a certain degree, the action of an excess of acid or of base (Art. V. No. 4); as well as that of another acid, or a foreign base. The only difference between them in this respect depends on the force of cohesion, which may act more or less strongly to produce crystallisation, which is peculiar to certain proportions of the acid and its base, probably in consequence of the figure assumed by the particles of which the combination consists.

5. When an acid has the property of forming a precipitate, by combining with a base, it has been supposed to possess a greater affinity for that base than the acid with which it was before combined, without even examining to what point the new acid may have effected the decomposition; and without considering that an opposite decomposition would have taken place merely by a change of the proportions, and might consequently in that case have justified an opposite conclusion.

In this manner as the tartareous acid has the property of forming, with pot-ash, an acidulous salt of difficult solubility, and consequently affords a precipitate with all those salts that have pot-ash for their base, and are not diluted with too great a quantity of water, it has been supposed to have a greater affinity for pot-ash than the other acids. Bergman has made an exception with respect to the sulphuric acid, because he supposed that the tartareous acid could only act upon that pot-ash which exceeds the combination of the acidulous sulphate of pot-ash; a supposition which I trust has been refuted in the preceding numbers. He has also excepted the nitric and muriatic acids, because he imagines that the tartareous acid acts, with respect to the nitrate and muriate of pot-ash, in the same manner as with the sulphate; and nevertheless he has not ascertained the existence of an acidulous nitrate and muriate of pot-ash similar to the acidulous sulphate of pot-ash.

He also concludes, from experiments made with the salts which have soda for their base, without however making known these experiments, that the tartareous acid ought to be placed after the oxalic acid; but, these exceptions apart, the tartareous acid, according to him, completely decomposes all the other salts that have a fixed alkali for their base.

What embarrasses Bergman is, that the tartareous acid does not produce a precipitate with the salts which have soda for their base. He believes that this apparent difference is a consequence of the soda not having the property of forming a salt of sparing solubility with an excess of acid; but then the indication which is thought to afford evidence of the decomposition is wanting; and it becomes necessary to be satisfied with the probability, that the affinities of one fixed alkali follow the same order as those of the other.

All this classification of affinities is founded upon the false supposition, that one acid will expel another from its combinations by its affinity alone, considered as a constant force; and this supposition necessarily demands several others, in order to explain as exceptions, those facts which are naturally derived from a general property.

6. I have examined the decomposition of the acidulous tartrite of pot-ash, by the nitric acid, which, according to the received opinions, then also adopted by me, ought to decompose it, by completely depriving it of its base. I therefore put some acidulous tartrite of pot-ash and nitric acid in digestion together, and obtained by cooling, some fine crystals of nitrate of

of pot-aſh. I repeated the experiment ſeveral times, by adding nitric acid till nitrate of pot-aſh was no longer ſeparated. I then expoſed the liquid to heat capable of making the diſengaged portion of nitric acid evaporate, without altering the tartareous acid. After this the liquid had acquired an oily conſiſtence; it had no ſmell, and did not announce the exiſtence either of the nitric acid or of pot-aſh; but on expoſing it to a ſtrong heat, much nitrous gas was diſengaged, the tartareous acid was reduced to coal, and its aſhes afforded a conſiderable quantity of carbonate of pot-aſh.

7. In this operation one portion of nitrate of pot-aſh is ſeparated by the force of cryſtalliſation in that ſalt, as far as that point at which that force is overcome by the ſuperabundant acid. The acidulous tartar is rendered ſoluble by the action of the nitric acid, which, at the ſame time, deprives it, by cryſtalliſation, of a part of the baſe neceſſary to make it inſoluble.

On the other hand, the tartareous acid, poured on a ſolution of the nitrate of pot-aſh, deprives it of its pot-aſh to a certain degree, and forms an acidulous tartarite which is precipitated; but as it has not the property of forming an acidulous tartarite of ſoda, of ſparing ſolubility, it does not produce any precipitate with thoſe ſalts that have ſoda for their baſe.

In both circumſtances, all that portion which cannot be ſeparated by the force of coheſion, forms a liquid, in which the ſubſtances act in proportion to their preſent maſſes.

Nothing, therefore, can be concluded about the reſpective affinity from ſuch ſeparations as are effected by precipitation or cryſtalliſation, ſince by the mere change of proportions, oppoſite decompoſitions may frequently be obtained.

8. The precipitations which take place when, on comparing the affinities of baſes, one of them forms an inſoluble combination, has given riſe to an error of the ſame kind; and on this ground alone it has been eſtabliſhed that lime has a greater affinity than alkali for the fluoric, phoſphoric, and arſenical acids, or, in a word, with all thoſe which form with it an inſoluble compound; and that conſequently it has the property of entirely decompoſing ſalts formed by the alkali and theſe acids. This precipitation is not the reſult of elective affinity, and it is not complete; but its quantity is determined by the proportion which the action of the fluid bears to the force of coheſion in the precipitate: hence it is that the precipitate is frequently diſſolved again on increaſing the quantity of the liquid ſubſtance oppoſed to it.

9. Although Bergman has well deſcribed the changes which heat is capable of producing in chemical action, when the ſubſtances have a tendency to become volatile, and has alſo warned us againſt employing too ſtrong a heat in evaporation, the whole influence of heat in thoſe operations by which ſalts are ſeparated, in order to form a judgment of their affinities, has not yet been ſhewn.

It would not have been concluded that the ſulphuric acid has more affinity for the fixed alkalis than the nitric or muriatic acids, from the ſimple circumſtance that, by a ſtrong heat,

heat, it expels thofe acids from their combinations; if attention had at the fame time been paid to the fact, that even by the heat employed for caufing the evaporation and obtaining the cryftallifation of falts, the proportions of the volatile acids may be confiderably changed by the fulphuric acid which remains oppofed to them, and which may even at laft expel them entirely, by means of the difference which exifts between its fixity and that of thofe acids. (Art. VII. No. 5).

10. We are indebted to Bergman for fome ufeful obfervations relative to the errors that may originate from the folubility of a fubftance which is difengaged, and of which the feparation is not perceived. He remarks that pot-afh or foda do not affect the tranfparence of a folution of falt with bafes of lime, if this folution be diluted with fifty times its quantity of water; becaufe the lime which is feparated, being foluble, remains in the water. But he does not confider that if the lime did then poffefs only its natural folubility, this would be a very feeble obftacle to the precipitation; for it would require nearly feven hundred parts of water to effect its folution. What adds fo greatly to its natural folubility is, that it continues to remain in combination with the acid (Art. V. No. 5), and that it cannot be feparated without retaining a part of this acid (Art. III. No. 9), by which its folubility is fo increafed.

11. Notwithftanding his general obfervations, Bergman has miftaken the effects of folubility in a variety of circumftances. For inftance, he did not believe that the nitric and muriatic acids were capable of acting upon the phofphate of lime, though the only difference that can be afcertained in this refpect between thefe acids and the fulphuric, without being acquainted with the comparative force of their affinities, is, that the two firft-mentioned only form foluble combinations, while that produced by the fulphuric acid may in a great meafure be withdrawn by the force of cryftallifation.

12. It is the folubility of lime, as well as of barites, acquired by the action of the acid upon thofe earths, which prevents ammonia from affording any precipitate in the folution of thofe falts of which they are the bafe. Neverthelefs the firft portion of ammonia which is mixed, for inftance, with the muriate of lime, emits fcarcely any fmell, which indicates that it has entered into combination; and its action may be rendered perceptible, as we fhall fhew.

I mixed fome ammonia with a folution of muriate of lime, and evaporated the liquid in a retort. When it was reduced to a certain point, a confiderable quantity of precipitate was formed. I continued the experiment: towards the end the quantity of precipitate was much diminifhed: a pellicle was formed, and by cooling a great quantity of cryftals in needles of a tolerable length were obtained. It was a triple falt, from which ammonia might be difengaged by lime. This falt, on being again diffolved and evaporated in the open air, no longer afforded indications of ammonia when tried by lime.

It is therefore evident, that when the water was not too abundant, the ammonia precipitated part of the lime, though it was rendered much more foluble by the acid, and though the action of the ammonia was confiderably weakened by heat, which diminifhed both

both its affinity and its quantity. In proportion as the latter was reduced, the precipitate was again diſſolved; notwithſtanding which, there were ſome remains of ammonia after a long evaporation, and it was only by means of the action of the air that it was entirely diſſipated. The ſeparation of the lime would doubtleſs become much more ſenſible, if ammoniacal gas were received in a ſlightly diluted ſolution of the muriate of lime.

When ammonia produces a precipitate with the ſalts that have alumine for their baſe, it is becauſe this earth is leſs ſoluble than lime, even when it is combined with the portion of acid which it retains when precipitated.

ARTICLE XII.

On Complex Affinities.

1. I ſhall proceed to examine, under the more general name of *complex affinity*, what has been conſidered as due to the concurrence of four affinities, and which has been commonly known by the name of *double affinity*.

To give an idea of the action of four affinities, Bergman examined the effect which takes place on mixing the ſolution of the ſulphate of pot-aſh with that of the muriate of lime. It is, ſays he, the ſame as if the proportions of ſulphuric and muriatic acid, of lime and pot-aſh, which entered into the compoſition of theſe ſalts, were put in the quantity of water employed; the two baſes act by their affinities upon the two acids: but though the affinity of the pot-aſh for the ſulphuric acid be ſtronger than what it has for the muriatic acid, yet the affinity of the latter for the pot-aſh, added to the affinity of the ſulphuric acid for the lime, gives an aggregate of forces greater than the affinity of the ſulphuric acid for the pot-aſh, and that of the muriatic acid for lime: thus an exchange of baſes is determined; ſo that inſtead of ſulphate of pot-aſh and muriate of lime, we have ſulphate of lime and muriate of pot-aſh. This explanation is always founded on the ſuppoſition that the affinities are conſtant powers, independently of the quantities and ſtate of ſaturation.

2. When two baſes act together upon an acid, the latter is divided, or rather divides its action in proportion to their maſſes; and if inſtead of one we have two acids, and no ſeparation is effected either by precipitation or cryſtalliſation, both the acids will act equally upon the two baſes in the proportion of their maſſes. If each of theſe acids were originally combined with a baſe, the amount of the mutual forces of the acids and alkalis, after mixing the ſolutions of the ſalts, will be the ſame as before. No muriate of pot-aſh nor ſulphate of lime are formed; but there will be a combination of pot-aſh, lime, ſulphuric acid, and muriatic acid, which will give the ſame degree of ſaturation as before the mixture. Hence it is, that on mixing two ſalts, which, by exchange, ought to produce combinations that would have very different proportions of principles, there is not, as has been juſtly remarked by Guyton, either the acidity or the alkalinity which would neceſſarily appear if ſuch a change were really to take place.

3. The exchange of bafes has been infered merely from the refult of the precipitation and cryftallifation which have been obferved; but this effect has not been attributed to its real caufe.

In Art. V. it has been fhewn that the force of cohefion determines the feparation which, in the elective affinities, takes place by precipitation or cryftallifation. It is alfo the fame force which produces the fame effect in the complex affinities. When I mix the folution of fulphate of pot-afh with that of the muriate of lime, the quantity of water not being great, the lime, in its contact with the fulphuric acid (No. 1), is fubject to the effect of the force of cohefion in a higher degree than the pot-afh. Thus a new force is added to thofe which previoufly exifted; and this muft determine the combination of the fulphuric acid with the lime, at the fame time as its precipitation.

4. If we take a rapid view of all the known decompofitions which are owing to the complex affinities, and we fhall fee that it is always to the fubftances which have the property of forming a precipitate, or a falt that may be feparated by cryftallifation, that an excefs of affinity has been attributed beyond thofe oppofed to them: fo that we may foretel by the degree of folubility of the falts which are formed in a liquid, which are the fubftances of which Bergman and other learned chemifts have pretended to reprefent the forces in their fymbolical tables, by always attributing a fuperiority of affinity to the two fubftances which are difpofed to form an infoluble combination relative to the quantity of the folvent.

Lime, magnefia, barites, and ftrontian, form, with the carbonic acid, infoluble falts; all the foluble combinations of thefe earths, when mixed with carbonates of alkalis, produce an exchange, from which the formation and precipitation of carbonates with earthy bafes refult.

Barites forms, with the fulphuric acid, an infoluble falt. Every time the folution of a fulphate is mixed with that of a falt with bafe of barites, a falt having a fimilar bafe, is formed and precipitated.

As lime makes a fparingly foluble fulphate, which is in a great meafure precipitated, if there be not much water, it alfo changes its bafe with all the foluble fulphates, till the period when precipitation ceafes, by the folubility of the fulphate of lime. The fulphate of lime being ftill more foluble than the fulphate of barites, the more foluble falts with bafe of barites decompofe the fulphate of lime.

The oxide of filver forms an infoluble falt with the muriatic acid: all the falts of filver which are foluble, being mixed with foluble muriates, the muriate of filver is precipitated. Mercury, which is not too much oxided, acts in the fame manner.

As the muriate of lead is fcarcely foluble, the falts which the oxide of lead forms with the other acids, and which poffefs folubility, produce a precipitate with the foluble muriates; but as it makes an infoluble falt with the fulphuric acid, the folution of muriate of lead affords a precipitate of fulphate of lead, when mixed with foluble fulphates.

5. When

5. When, therefore, water is evaporated, in which ſeveral ſalts have been put in a ſtate of ſolution, they will be found to ſeparate according to the order of their ſolubility, and it is from this property that we are able to aſcertain the changes of baſe which may be effected.

But the ſolubility of ſalts varies according to the temperatures. It is, therefore, the ſolubility relative to the different temperatures which ought to be conſidered. The nitrate of pot-aſh, mixed with the muriate of ſoda, will cryſtalliſe at a low temperature; but the muriate of ſoda will ſeparate even during the time of evaporation. No exchange of baſes will be effected, becauſe the nitrate of ſoda is a little more ſoluble in cold than the nitrate of pot-aſh, and on the contrary, the muriate of pot-aſh is rather more ſoluble in heat than the muriate of ſoda.

6. I here attend only to the principal reſult, ariſing from a force of coheſion ſo great, that it cauſes the effect of the forces oppoſed to it to diſappear. But when this power is not conſiderable in the combinations that may be formed, the mutual action of the ſubſtances which remain in the liquid ſtate, that of the ſolvent, and the proportions which vary by the cryſtalliſation of a combination that is ſeparated, muſt produce different effects. The experiments I have began will not fail to elucidate this ſubject.

7. Another circumſtance may change the action of the complex affinities; namely, the formation of a triple ſalt which precipitates; but by knowing the degree of ſolubility of this combination, we may ſtill foretel the decompoſition that will take place. The ſame conſideration is applicable to the affinities called *elective.*

8. A precipitate is ſometimes effected by the mixture of two ſaline ſubſtances which have the ſame acid; for inſtance, by a mixture of the muriate of magneſia and the muriate of lime. It is probable that two combinations are then formed, one with an exceſs of acid and a ſmall part of the two baſes, the other with the greateſt part of the two baſes and a ſmall portion of acid.

This effect is analogous to what we have obſerved in Art. IX. No. 3; but here it is the mutual affinity of the two baſes which determines the precipitation.

9. We have ſeen in Art. VII. that heat, by increaſing the volatility of a ſubſtance, weakens its combination. This cauſe acts no leſs in the complex than in the elective affinities. It is a force added to thoſe which act, and determines the union and the ſeparation of thoſe ſubſtances which have the greateſt tendency to form a volatile combination.

Whenever, therefore, it is deſired to know what will happen, by expoſing two ſalts to the action of heat, it is only neceſſary to examine whether one of the two baſes and one of the two acids poſſeſs a greater degree of volatility than the remaining baſe and acid, and we may be aſſured that, on applying a ſufficient degree of heat, the combination of the moſt volatile baſe and acid will be formed and ſublimed, while the more fixed baſe and acid will alſo remain combined. Among the baſes, ammonia and the oxide of mercury, and among the acids, the carbonic and muriatic, afford ſeveral illuſtrations of this truth.

10. Efflorefcence ought alfo to be confidered as a force which, in the complex affinities, may determine a combination poffeffing this property, and to this is owing the formation of natron in the valley of the Lake of Natron, and other places where the fame circumftances occur.

The obfervations which I have prefented to the Inftitute of Egypt, which will form a fequel to the interefting difcovery of the valley of the Lakes of Natron, for which we are indebted to General Andreoffi, prove that the circumftances neceffary to the formation of natron are, 1. a fand, which contains much carbonate of lime; 2. moifture; and, 3. the prefence of the muriate of foda: I have alfo remarked that the ftalks of the reed in a great degree facilitate the production of this fubftance. I have promifed to explain the formation of the carbonate of foda by means of thefe circumftances, which I fhall now endeavour to perform.

Calcareous fand, conftantly impregnated with moifture, may be confidered as a folution of the muriate of foda, which acts upon the carbonate of lime. Now it follows, from what has been explained in Art. IV. that infolubility caufes a great diminution in the mutual action of a folid and a liquid fubftance, but that it does not deftroy it. This action is oppofed to the infolubility of the carbonate of lime, which is not abfolute. A folution muft therefore be formed of a fmall quantity of carbonate of lime, and confequently (No. 1 and 2), the conftituent parts of this carbonate and of the muriate of foda, which are in folution, exert a reciprocal action; otherwife the prefence of the carbonate of lime would not be a condition neceffary to the formation of the carbonate of foda*.

We muft, therefore, confider the humidity of the calcareous fand, in which the carbonate of foda is formed, as a folution of muriate of foda and a fmall quantity of carbonate of lime: hence the foda is found in the prefence of the carbonic acid, and the efflorefcence, which is a property of the carbonate of foda, ought to be confidered as a new force that tends to remove it from this combination. In fhort, when, in a foil impregnated with muriate of foda, we meet with the ftalks of reeds which favour the efflorefcence, the carbonate of foda not only accumulates round thefe ftalks, but fometimes it is not formed

* The folution of the carbonate of lime by the muriate of pot-afh, and by the fulphate of pot-afh, which indubitably act like the muriate of foda, has been proved by a direct experiment, for which we are indebted to Guyton (*Mem. de Scheele*, part II. note de la page 18). "The folution of fulphate of pot-afh, "muriate of pot-afh, &c. poured in lime water which has been rendered milky by water impregnated with "carbonic acid gas, immediately caufed the precipitate to difappear. There was likewife no earthy preci-"pitate when water, charged with carbonic acid gas, was poured in a mixture of lime water and a folution "of thefe neutral falts; the liquor always contained a portion of uncombined alkali." Guyton combats the opinion of Scheele, who did not obferve any decompofition with the muriate and fulphate with bafe of pot-afh, but only with the falts with bafe of foda.

The difference of opinion between thofe celebrated chemifts arofe from the circumftance that one afcertained the decompofition only by the efflorefcence, which is peculiar to falts with bafe of foda (Art. VIII.) while the other obferved it in a liquid; but in the latter inftance it is much more limited.---B.

4 without

without such assistance, when certain circumstances, such as a too argillaceous nature of the soil, &c. are little favourable to its production; so that at a short depth muriate of soda only is discoverable.

I wished to ascertain the truth of this explanation in a trough placed in one of the gardens belonging to the Institute. For this purpose some carbonate of lime and siliceous earth, both well washed, were mixed together, to which was added a certain proportion of muriate of soda: a hole was made in this mixture, for the purpose of pouring in water occasionally, and keeping up the necessary degree of moisture. An incrustation of muriate of soda is formed at the surface, which already strongly changes the colour of paper tinged with fernambuc, like the alkalis; but we cannot expect to obtain an efflorescence so considerable as to be perceptible to the sight, till after a much greater length of time.

11. The preceding observations shew that the only difference which distinguishes the complex affinities from those called *elective*, is, that in the former, substances are brought into action which are nearly in an uniform degree of saturation; and that in the latter, there is a substance present which is not yet saturated (or several such substances may be present); so that in the former, a new degree of saturation is established only in proportion to the combinations which are capable of being separated; whereas, in the latter, the action of the unsaturated substances becomes in equilibrio with that of the substances which were already so; whence it happens that the force of cohesion and that of elasticity produce their effect more completely in the complex than in the elective affinities.

VI.

Experiments on the Solar and on the Terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them are subject, in order to determine whether they are the same or different. By WM. HERSCHELL, LL.D. F.R.S.*

IN the former part of this paper, of which an abridgment was given at page 360 of our last volume, it was shewn that heat derived immediately from the sun, or from ignited terrestrial substances, is occasioned by rays emanating from them, and that such rays are subject to the laws of reflection and of refraction. The similarity between light and heat was the most remarkable feature of the experiments there related. In the present memoir the author proceeds to point out some striking and substantial differences in the rays of light and heat which afford data for discussing whether there two effects be produced by one and the same power.

* Abridged from the Philosophical Transactions, 1800, page 437.

The

The various refrangibility of the rays of heat from those of light was shewn by those experiments with the prism, which undoubtedly constitutes a great point of resemblance. But he points out a very material difference, consisting in the much greater range over which the rays of heat are dispersed when compared with those of light. He has exhibited this by delineating a spectrum of light of a certain assumed length represented by the line G Q (in Fig. 5, Plate III.), divided into seven parts, according to the dimensions assigned to the colours by Newton; and to this he has drawn perpendicular lines, representing the intensity of illumination possessed by the light at each particular part. The length of the maximum perpendicular being arbitrary, he has assumed it to be $\frac{27}{33}$ of the whole line, namely, L R at the confines of the yellow and green. The lengths of the other ordinates to the curve, namely, K M H I N O P, are also determined in relative length and position from his former experiments.

To find a figure in the same manner to express the heating power of the refracted prismatic rays, which may be called the spectrum of heat, he examined the extent of the invisible rays, and found that its line was in proportion to that of the visible spectrum as 2 to 3, or rather, as some allowance must be made beyond the last ordinate, as $2\frac{1}{4}$ to 3. Whence the whole calorific spectrum proves to be as $5\frac{1}{4}$ to 3, with regard to the luminous spectrum. And if the maximum of heat be expressed by a line of the same length as that which denotes the maximum of light, and other ordinates be drawn to express the proportions of degrees indicated by the thermometer at the various points of the whole spectrum, another curve B C D E F G H I K L M N O P will be obtained to express the intensities of heat. From the mere inspection of these two figures, thus drawn as lying upon one another, it is seen how very differently the prism disperses the heat making rays and those which occasion illumination. These rays neither agree in their mean refrangibility nor in the situation of their maxima. At R, where there is most light, there is but little heat, and at S, where there is most heat, there is no light at all!

Experiment 21. *The sines of refraction of the heat making rays are in a constant ratio to the sines of incidence.* A prism was placed half an inch beyond the last visible red colour of the solar spectrum from a prism, and another at the distance of an inch, while a third was placed beside the spectrum as a standard. The two first exhibited change of temperature, as in the former trials of this kind, while the last underwent no change. Other experiments, with the like event, were tried with prisms of different refracting angles and of different kinds of glass, and also of water. Hence, as the visible part of the rays has long been determined to have the constant ratio here mentioned, and the invisible part always bore the same relative position to these, it must follow that that part also is deflected according to the same law.

Experiment 22. *The different refrangibility of heat is corrected by contrary refraction in different mediums.* A prism of crown glass, having an angle of 25 degrees, another of flint glass, with an angle of 24, and a third of crown glass, with an angle of 10 degrees, being

being put together as they are placed when experiments of achromatic refractions are to be made, were found to give a spectrum nearly without colour. The thermometer being placed in the middle of this spectrum, had its temperature raised two degrees; but a thermometer placed near either end of the spectrum was not affected. This result shews that the different refrangibility of heat, as well as that of light, admits of prismatic correction, and it also confirms the constant ratio of the sines of refraction to those of incidence.

Experiment 23. *In burning glasses the focus of the rays of heat is different from the focus of the rays of light.* A burning lens was placed in the rays of the sun, its aperture being reduced to three inches, in order to lessen aberration. The place of the luminous focus was ascertained by throwing hair powder into the air with a puff. A stick of sealing wax was then held 1″, 6 or 4 beats of a chronometer in the contracted pencil half an inch nearer to the lens than the focus. In this time no impression was made upon the wax. It was then applied half an inch farther from the lens than that focus, and in eight-tenths of a second or two beats of the same chronometer it was considerably scorched. When the sealing wax was also exposed to the focus of light, it was equally affected in the same time. Whence the Doctor apprehends we may safely conclude, notwithstanding the little accuracy to be expected from so coarse an experiment, that the focus of heat was certainly farther from the lens than the focus of light, and probably not less than one quarter of an inch; the heat at half an inch beyond the focus of light being still equal to that in the focus itself.

In the next place our author proceeds to the subject of the transmission of heat through diaphanous bodies. The experiments made by the prism, the lens, and the mirror, are subject to certain natural imperfections, which must impair the progress of scientific induction. The prism refracts, reflects, transmits, and scatters them at the same time, and the laws by which it acts in every one of these operations require to be investigated. A lens not only partakes of the errors of the prism, but also is subject to the aberration from its spherical figure. And a mirror, besides its natural incapacity of separating the rays of light from those of heat, scatters them very prosusedly; but our author adds that the scanty provision of materials to act upon these rays has been partly our own fault, as every diaphanous body may become a new tool in the hands of a diligent observer.

(*To be continued.*)

VII.—*Outlines*

VII.

Outlines of Experiments and Inquiries respecting Sound and Light. By THOMAS YOUNG *M. D. F. R. S.* *

IT has long been my intention to lay before the Royal Society a few obſervations on the ſubject of ſound; and I have endeavoured to collect as much information, and to make as many experiments, connected with this inquiry, as circumſtances enabled me to do; but, the further I have proceeded, the more widely the proſpect of what lay before me has been extended; and, as I find that the inveſtigation, in all its magnitude, will occupy the leiſure hours of ſome years, or perhaps of a life, I am determined, in the mean time, leſt any unforeſeen circumſtances ſhould prevent my continuing the purſuit, to ſubmit to the Society ſome concluſions which I have already formed from the reſults of various experiments. Their ſubjects are, I. The meaſurement of the quantity of air diſcharged through an aperture. II. The determination of the direction and velocity of a ſtream of air proceeding from an orifice. III. Ocular evidence of the nature of ſound. IV. The velocity of ſound. V. Sonorous cavities. VI. The degree of divergence of ſound. VII. The decay of ſound. VIII. The harmonic ſounds of pipes. IX. The vibrations of different elaſtic fluids. X. The analogy between light and ſound. XI. The coaleſcence of muſical ſounds. XII. The frequency of vibrations conſtituting a given note. XIII. The vibrations of chords. XIV. The vibrations of rods and plates. XV. The human voice. XVI. The temperament of muſical intervals.

I. *Of the Quantity of Air diſcharged through an Aperture.*

A piece of bladder was tied over the end of the tube of a large glaſs funnel, and punctured with a hot needle. The funnel was inverted in a veſſel of water; and a gage, with a graduated glaſs tube, was ſo placed as to meaſure the preſſure occaſioned by the different levels of the ſurfaces of the water. As the air eſcaped through the puncture, it was ſupplied by a phial of known dimenſions, at equal intervals of time; and, according to the frequency of this ſupply, the average height of the gage was ſuch as is expreſſed in the firſt Table. It appears, that the quantity of air diſcharged by a given aperture, was nearly in the ſubduplicate ratio of the preſſure; and that the ratio of the expenditures by different apertures, with the ſame preſſure, lay between the ratio of their diameters and that of their areas. The ſecond, third, and fourth Tables ſhow the reſult of ſimilar experiments, made with ſome variations in the apparatus. It may be inferred, from comparing the experiments on a tube with thoſe on a ſimple perforation, that the expenditure is increaſed, as in water, by the application of a ſhort pipe.

* Philoſ. Tranſactions, 1800, p. 106.

Table

Table I.

A	B	C
.00018	.25	3.9
.00018	.58	11.7
.00018	1.	15.6
.001	.045	7.8
.001	.2	15.6
.001	.7	31.2
.004	.35	46.8

A is the area, in ſquare inches, of an aperture nearly circular. B, the preſſure in inches. C, the number of cubic inches diſcharged in one minute.

All numbers throughout this paper, where the contrary is not expreſſed, are to be underſtood of inches, linear, ſquare, or cubic.

Table II.

A	B	C
.07	1.	2000.
.07	2.	2900.

A is the area of the ſection of a tube about two inches long. B, the preſſure. C, the quantity of air diſcharged in a minute, by eſtimation.

Table III.

A	B	C	D
.0064	1.15	.2	46.8
.0064	10.	.45	46.8
.0064	13.5	.35	31.2
.0064	13.5	.7	46.8

A is the area of the ſection of a tube. B, its length. C, the preſſure. D, the diſcharge in a minute.

Table IV.

A	B	C
.003	.28	46.8

A is the area of an oval aperture, formed by flattening a glaſs tube at the end: its diameters were .025 and .152. B, the preſſure. C, the diſcharge.

II. *Of the Direction and Velocity of a Stream of Air.*

An apparatus was contrived for meaſuring, by means of a water-gage communicating with a reſervoir of air, the preſſure by which a current was forced from the reſervoir through a cylindrical tube; and the gage was ſo ſenſible, that, a regular blaſt being ſupplied from the lungs, it ſhewed the ſlight variation produced by every pulſation of the heart. The current of air iſſuing from the tube was directed downwards, upon a white plate, on which a ſcale of equal parts was engraved, and which was thinly covered with a coloured liquid; the breadth of the ſurface of the plate laid bare, was obſerved at different diſtances from the tube, and with different degrees of preſſure, care being taken that the liquid ſhould be ſo ſhallow as to yield to the ſlighteſt impreſſion of air. The reſults are collected in

in Tables v. and vi. and are exhibited to the eye in Plate IV. Figs. 1—12. In order to meaſure with greater certainty and preciſion, the velocity of every part of the current, a ſecond cavity, furniſhed with a gage, was provided, and pieces perforated with apertures of different ſizes were adapted to its orifice: the axis of the current was directed as accurately as poſſible to the centres of theſe apertures, and the reſult of the experiments, with various preſſures and diſtances, are inſerted in Tables vii. viii. and ix. The velocity of a ſtream being, both according to the commonly received opinion and to the experiments already related, nearly in the ſubduplicate ratio of the preſſure occaſioning it, it was inferred, that an equal preſſure would be required to ſtop its progreſs, and that the velocity of the current, where it ſtruck againſt the aperture, muſt be in the ſubduplicate ratio of the preſſure marked by the gage. The ordinates of the curves in Figs. 13—23, were therefore taken reciprocally in the ſubduplicate ratio of the preſſure marked by the ſecond gage to that indicated by the firſt, at the various diſtances repreſented by the abſciſſes. Each figure repreſents a different degree of preſſure in the firſt cavity. The curve neareſt the axis, is deduced from obſervations in which the aperture oppoſed to the tube was not greater than that of the tube itſelf; and ſhows what would be the diameter of the current, if the velocities of every one of its particles in the ſame circular ſection, including thoſe of the contiguous air, which muſt have acquired as much motion as the current has loſt, were equal among themſelves. As the central particles muſt be ſuppoſed to be leſs impeded in their motion than the ſuperficial ones, of courſe, the ſmaller the aperture oppoſed to the centre of the current, the greater the velocity ought to come out, and the ordinate of the curve the ſmaller; but, where the aperture was not greater than that of the tube, the difference of the velocities at the ſame diſtance was ſcarcely perceptible. When the aperture was larger than that of the tube, if the diſtance was very ſmall, of courſe, the average velocity came out much ſmaller than that which was inferred from a ſmaller aperture; but, where the ordinate of the internal curve became nearly equal to this aperture, there was but little difference between the velocities indicated with different apertures. Indeed, in ſome caſes, a larger aperture ſeemed to indicate a greater velocity: this might have ariſen in ſome degree from the ſmaller aperture not having been exactly in the centre of the current; but there is greater reaſon to ſuppoſe, that it was occaſioned by ſome reſiſtance derived from the air returning between the ſides of the aperture and the current entering it. Where this took place, the external curves, which are ſo conſtructed as that their ordinates are reciprocally in the ſubduplicate ratio of the preſſure obſerved in the ſecond cavity, with apertures equal in ſemidiameter to their initial ordinate, approach, for a ſhort diſtance, nearer to the axis than the internal curve: after this, they continue their courſe very near to this curve. Hence it appears, that no obſervable part of the motion diverged beyond the limits of the ſolid which would be formed by the revolution of the internal curve, which is ſeldom inclined to the axis in an angle ſo great as ten degrees. A ſimilar concluſion may be made, from obſerving the flame of a candle ſubjected to the action of a blowpipe: there is no divergency beyond the narrow limits of the current; the

flame, on the contrary, is every where forced by the ambient air towards the current, to supply the place of that which it has carried away by its friction. The lateral communication of motion, very ingeniously and accurately observed in water by Professor Venturi, is exactly similar to the motion here shown to take place in air; and these experiments fully justify him in rejecting the tenacity of water as its cause: no doubt it arises from the relative situation of the particles of the fluid, in the line of the current, to that of the particles in the contiguous strata, which is such as naturally lead to a communication of motion nearly in a parallel direction; and this may properly be termed friction. The lateral pressure which urges the flame of a candle towards the stream of air from a blowpipe, is probably exactly similar to that pressure which causes the inflection of a current of air near an obstacle. Mark the dimple which a slender stream of air makes on the surface of water; bring a convex body into contact with the side of the stream, and the place of the dimple will immediately show that the current is inflected towards the body; and, if the body be at liberty to move in every direction, it will be urged towards the current, in the same manner as, in Venturi's experiments, a fluid was forced up a tube inserted into the side of a pipe through which water was flowing. A similar interposition of an obstacle in the course of the wind, is probably often the cause of smoky chimneys. One circumstance was observed in these experiments, which it is extremely difficult to explain, and which yet leads to very important consequences: it may be made distinctly perceptible to the eye, by forcing a current of smoke very gently through a fine tube. When the velocity is as small as possible, the stream proceeds for many inches without any observable dilatation; it then immediately diverges at a considerable angle into a cone, Plate IV. Fig. 24; and, at the point of divergency, there is an audible and even visible vibration. The blowpipe also affords a method of observing this phenomenon: as far as can be judged from the motion of the flame, the current seems to make something like a revolution in the surface of the cone, but this motion is too rapid to be distinctly discerned. When the pressure is increased, the apex of the cone approaches nearer to the orifice of the tube, Figs. 25, 26; but no degree of pressure seems materially to alter its divergency. The distance of the apex from the orifice, is not proportional to the diameter of the current; it rather appears to be the greater, the smaller the current, and is much better defined in a small current than in a large one. Its distance in one experiment is expressed in Table X, from observations on the surface of a liquid; in other experiments, its respective distances were sometimes considerably less with the same degrees of pressure. It may be inferred, from the numbers of Tables VII and VIII, that in several instances a greater height of the first gage produced a less height of the second: this arose from the nearer approach of the apex of the cone to the orifice of the tube, the stream losing a greater portion of its velocity by this divergence than it gained by the increase of pressure. At first sight, the form of the current bears some resemblance to the *vena contracta* of a jet of water: but Venturi has observed, that in water an increase of pressure increases, instead of diminishing, the distance of the contracted

 section

section from the orifice. Is it not possible, that the facility with which some spiders are said to project their fine threads to a great distance, may depend upon the small degree of velocity with which they are thrown out, so that, like a minute current, meeting with little interruption from the neighbouring air, they easily continue their course for a considerable time?

Table V.

A	1.	2.	3.	3.8
B	C	C	C	C
1.	.1	.1	.1	
2.	.12	.12	.2	
3.	.17	.25	.3	
4.	.2	.4	.4	
5.	.25	.5		
6.	.30	.52		
7.	.35	.54	.5	
8.	.37	.56		
9.	.39	.58		
10.	.40	.6	.6	.5
15.		.7		
18.	.50			
20.				

The diameter of the tube .07. A is the distance of the liquid from the orifice. B, the pressure. C, the diameter of the surface of the liquid displaced.

Table VI.

A	1.	2.
B	C	C
1.	.1	.1
2.	.13	
3.	.2	.2
4.	.25	.3
6.	.3	.4
7.	.35	.5
10.	.35	.6
15.	.35	.7
20.	.35	.7

Diameter of the tube, .1. A, B, and C, as in Table V.

Table VII.

A	.5	
B	.06	.15
C	D	D
.1	.083	
.2	.16	
.3	.25	.1
.4	.35	
.5	.45	
.6	.53	.2
.7	.6	
.8		.3
1.	.5	
1.2	.4	.4
1.5	.6	
2.	.67	.55
4.	1.3	1.
8.		2.
9.	.3	
14.	.5	

Diameter of the tube .06.

A is the distance of the opposite aperture, from the orifice of the tube. B, the diameter of the aperture. C, the pressure, indicated by the first gage. D, the height of the second gage.

Table

Table VIII.

A	.5				1.				2.				4.			
B	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	.5	.06	.15	.3	.5
C	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D
.1	.05	.05			.03				.017							
.2	.1	.1			.12	.08	.02		.034							
.5	.2	.22			.1	.00			.00							
1.	.32	.36	.1		.17	.1	.1	.05	.04							
2.	.52	.6	.2		.28	.22	.21	.08	.07							
3.	.8	.9	.3		.4	.36	.32	.12	.12		.1	.1				
4.	1.1	1.2	.4		.58	.52	.42	.16	.18		.15	.14				
5.		1.5	.5		.8	.68	.52	.2	.23		.2	.18	.04		.04	.05
6.		1.7	.6		1.	.83	.63	.25	.3		.25	.22	.05		.05	.06
7.		1.9	.7		1.2	1.	.75	.3	.35		.3	.26	.06		.06	.07
8.		2.1	.8		1.5	1.2	.88	.34	.4		.34	.3	.07		.07	.07
9.		2.3	.9		1.7	1.4	1.	.37	.45		.37	.34	.08		.08	.08
10.		2.6	1.		1.9	1.6	1.1	.4	.5		.4	.37	.09		.09	.09

Diameter of the tube .1. A, B, C, and D, as in Table VII.

Table IX.

A	1.15				3.3			4.
B	.15	.3	.5	1.	.06	.15	1.	.06
C	D	D	D	D	D	D	D	D
.5	.1	.1	.1					
1.	.2	.2	.2					
2.	.4	.35	.34	.13	.1	.1	.125	
3.	.6	.5	.5	.2	.15	.15	.18	.1

Diameter of the tube .3. A, B, C, and D, as in Table VII.

Table X.

A	B
.4	6.
.8	3.
1.2	1.5
1.8	1.
2.	.5
4.	.0

A is the preſſure. B, the diſtance of the apex of the cone from the orifice of a tube .1 in diameter.

III. *Ocular Evidence of the Nature of Sound.*

A tube about the tenth of an inch in diameter, with a lateral orifice half an inch from its end, filed rather deeper than the axis of the tube, Fig. 27, was inſerted at the apex of a conical cavity containing about twenty cubic inches of air, and luted perfectly tight: by blowing through the tube, a ſound nearly in uniſon with the tenor C was produced. By gradually increaſing the capacity of the cavity as far as ſeveral gallons, with the ſame mouth-piece, the ſound, although faint, became more and more grave, till it was no longer a muſical note. Even before this period a kind of trembling was diſtinguiſhable; and this, as the cavity was ſtill further increaſed, was changed into a ſucceſſion of diſtinct puffs, like the ſound produced by an exploſion of air from the lips; as ſlow, in ſome inſtances, as 4 or 3 in a ſecond. Theſe were undoubtedly the ſingle vibrations, which, when repeated with ſufficient frequency, impreſs on the auditory nerve the ſenſation of a continued ſound. On forcing a current of ſmoke through the tube, the vibratory motion

of

of the ſtream, as it paſſed out at the lateral orifice, was evident to the eye; although, from various circumſtances, the quantity and direction of its motion could not be ſubjected to exact menſuration. This ſpecies of ſonorous cavity ſeems ſuſceptible of but few harmonic ſounds. It was obſerved, that a faint blaſt produced a much greater frequency of vibrations than that which was appropriate to the cavity: a circumſtance ſimilar to this obtains alſo in large organ pipes; but, ſeveral minute obſervations of this kind, although they might aſſiſt in forming a theory of the origin of vibrations, or in confirming ſuch a theory drawn from other ſources, yet, as they are not alone ſufficient to afford any general concluſions, are omitted at preſent, for the ſake of brevity.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Galvanic Pile conſiſting of one and the ſame Metal throughout.

MR. Humphry Davy, who is at preſent attached to the Royal Inſtitution, has diſcovered, that the difference of the metals is not eſſential to the effect produced in the pile of Volta. From his former reaſoning on theſe important phenomena, (ſee our Journal IV. 380, 394) he was led to make trial of powers by which that difference of oxydation at the ſurfaces, which appears to be eſſential to the effect, and has hitherto ariſen from the habitudes of the metals themſelves, might be produced with ſimilar conſequences by changing the ſolvents. The metals hitherto tried each to form a pile by itſelf, and as far as my recollection ſerves are ſilver, copper, zinc, and lead; and one of the arrangements of the chemical agents is thus; a plate of metal, cloth ſoaked in diluted nitrous acid, cloth ſoaked in water, cloth ſoaked in ſulphuret of pot-aſh; then another plate of the ſame metal, and the three cloths as before: in which manner the repetitions may be continued at pleaſure. If a trough be uſed with cells, and the ſeparation between the acid and the ſulphuret of pot-aſh be made by a plate of horn inſtead of the cloth imbibed with water, the two fluids may be connected by a ſlip of wetted cloth hung over the upper edge of the horn. This will complete the communication without occaſioning any mixture, becauſe water is lighter than either of the other fluids. It is remarkable that the effects of this pile are the reverſe of what happens when the difference of action depends on the metals; that is to ſay, the extrication of hydrogen in the tube of communication, is made from the oppoſite wire to that from which it would have been made with the common pile.

Among the reflections and concluſions to which this valuable experiment muſt give riſe, it is not one of the leaſt important, that it overthrows the theory of the learned inventor Volta, who conſiders the current of electricity as being produced by the difference of conducting power in the different metals. And in this obſcure region of reſearch, though we are altogether unacquainted with the power by which the electricity is accumulated, it is one ſtep towards a true theory to have aſcertained, that one of thoſe already offered is without foundation.

Philoſophical

Philosophical Transactions of the Royal Society of London, for the Year 1800. *Part* III. *Quarto, p.* 295, *with twelve Plates.* London; sold by Elmsley. Price 17s. 6d.

THIS part contains:—19. Experiments on the solar, and on the terrestrial rays that occasion heat; with a comparative view of the laws to which light and heat, or rather the rays which occasion them, are subject, in order to determine whether they are the same or different. Part II. By William Herschel, L. L. D. F. R. S. 20. An Account of the Trigonometrical Survey, carried on in the years 1797, 1798, and 1799, by order of the Marquis Cornwallis, Master General of the Ordnance. By Captain William Mudge, of the Royal Artillery, F. R. S. Communicated by his Grace the Duke of Richmond, F. R. S.

Analytical Essays towards promoting the Chemical Knowledge of Mineral Substances. By Martin Henry Klaproth, Professor of Chemistry, Assessor to the Royal College of Physicians, Member of the Royal Academy of Sciences at Berlin, and various other learned Societies. Translated from the German. 1 vol. 8vo. p. 592. London. Sold by Cadell and Davies.

IT is altogether unnecessary to apprise the scientific world of the value of the analyses of this great improver of chemical science. I shall therefore only inform the reader in this place, that we are indebted for the present accurate and only translation to the learned editor of Gren's Chemistry, Dr. Gruber, who promises in his preface to give an immediate English edition of the next collection which Klaproth shall publish, and of which he has given hopes to the editor.

An Epitome of Chemistry in three Parts. Part I. intended to facilitate to the Student the Acquisition of Chemical Knowledge, by minute Instructions for the performance of Experiments. Part II. Directions for the Analysis of Mineral Waters; of Earths and Stones; of Ores of Metals; and of Mineral Bodies in general; and Part III. Instructions for applying Chemical Facts and reagents to various useful Purposes. By William Henry. Duodecimo, 216 Pages. Printed for Johnson.

THE title of this work is sufficiently copious to exhibit its objects and plan. The practical, as well as scientific knowledge of the author, have enabled him to produce a manual which I doubt not will be received with avidity by the public. I will not incur the imputation of vanity by assuming the office of an approver, where the author's powers are so well known: but I think I shall not deviate from the line of my duty in observing that, as far as my observation extends, he has, on the present occasion, exercised those powers with the diligence and attention which are peculiarly demanded in a manual of science.

Electric

Electric Shock by means of Galvanism.

IN anfwer to a letter on the fubject of the note at page 44, Mr. Cruickfhank has favoured me with an account of the experiment in which the common electric jar was charged by Galvanifm, fo as to give a perceptible fhock: a fact on the poffibility of which all doubt has, with refpect to myfelf, been removed by my firft experiments with talc, with filk, and with common air; a plate of which, over the condenfer of Volta, may be fo charged with Galvanifm, from a weak power, as to exhibit every electric intenfity that Bennet's electrometer is capable of, merely by varying the thicknefs of the ftratum of air.

Mr. Cruickfhank made his experiment as follows: A coated jar, whofe infide and outfide coatings were moiftened, was placed upon one of the extremities of the pile, and a wire connected with the infide coating, was then made to touch the other extremity. After this procefs the jar was taken in the hand which was wet, and the wire brought to touch the tongue. In this cafe, when the machine was very powerful, and gave ftrong fparks, he was feveral times confident that he received a flight fhock in the tongue, but this did not always fucceed, nor could he account for its failure. In truth, the reafoning on thefe facts by application of what we know of the laws of common electricity, is fufficiently obvious; but till we are better acquainted with the laws of electric condenfation in the pile, we muft expect anomalies tending to embarrafs our proceffes of deduction. I have met with feveral; of which like Mr. Cruickfhank, in the prefent inftance, I have deferred the publication till time fhould admit of repeating them with fuch variations as the facts them-felves have fuggefted.

From the fame private communication I copy the following curious extract:—" I ftill " ufe the new apparatus or troughs, which was defcribed in your Journal, and have never " once had occafion to remove the pieces. Indeed, when frefh filled with a folution of " common falt, to which a little muriatic acid is added, it is quite as powerful as when " firft constructed. I prefer the muriatic acid to the nitrous, as its effects are much " more permanent, and it does not in any fenfible degree act on the filver. The fhocks " given by the troughs are exactly fimilar to thofe of common electricity, and have none " of that grating or tearing fenfation fo remarkable in thofe from the pile. From two " troughs, containing in all 120 pair of pieces, a fpark of confiderable fize may be taken, " and the report or fnap may be heard in an adjoining room. The fpark may likewife " be taken from water, or from a conductor at a diftance, which I never could do with any " pile. If a point be prefented to the water, a curious hiffing is perceived, and a portion " of the fluid is inftantly converted into gas and vapour: I have fometimes diffipated a " whole drop in this way. I have likewife feen a very ftrong fpark run along a plate of " zinc, to get to water in a manner which I cannot yet explain: this is frequently ac-" companied with a hiffing noife. In fhort, I have obferved a number of curious facts " with this apparatus, which had efcaped notice, when I employed the piles only."

Galvanic Apparatus.

Fig. 1.

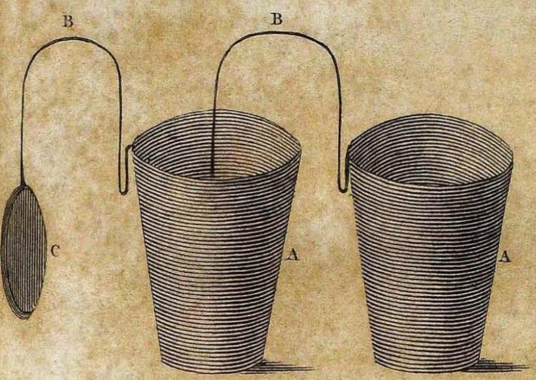


Fig. 2.

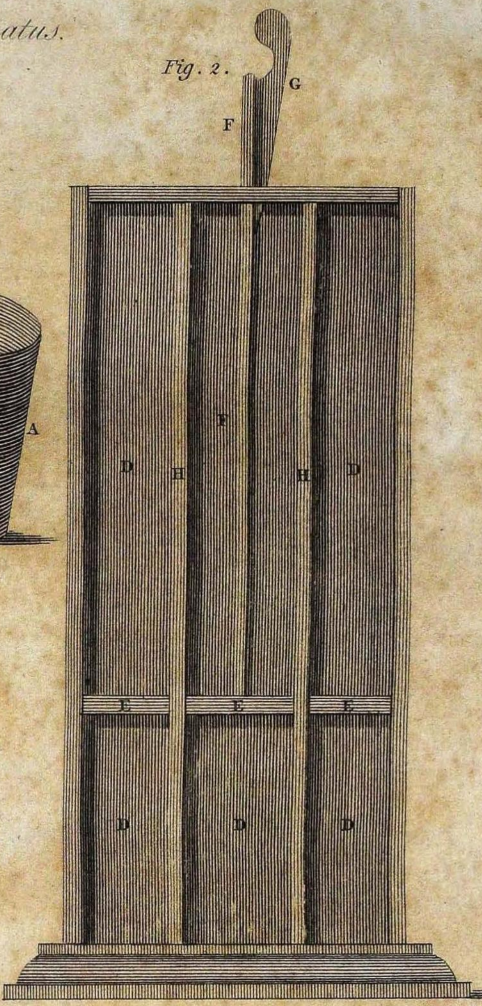


Fig. 3.

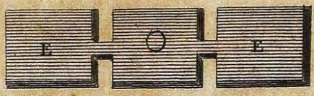
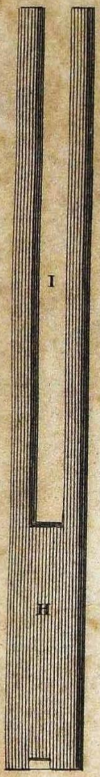
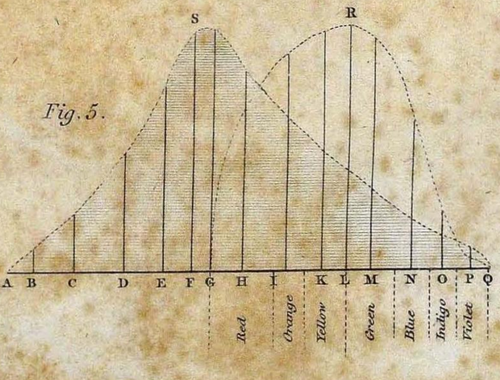
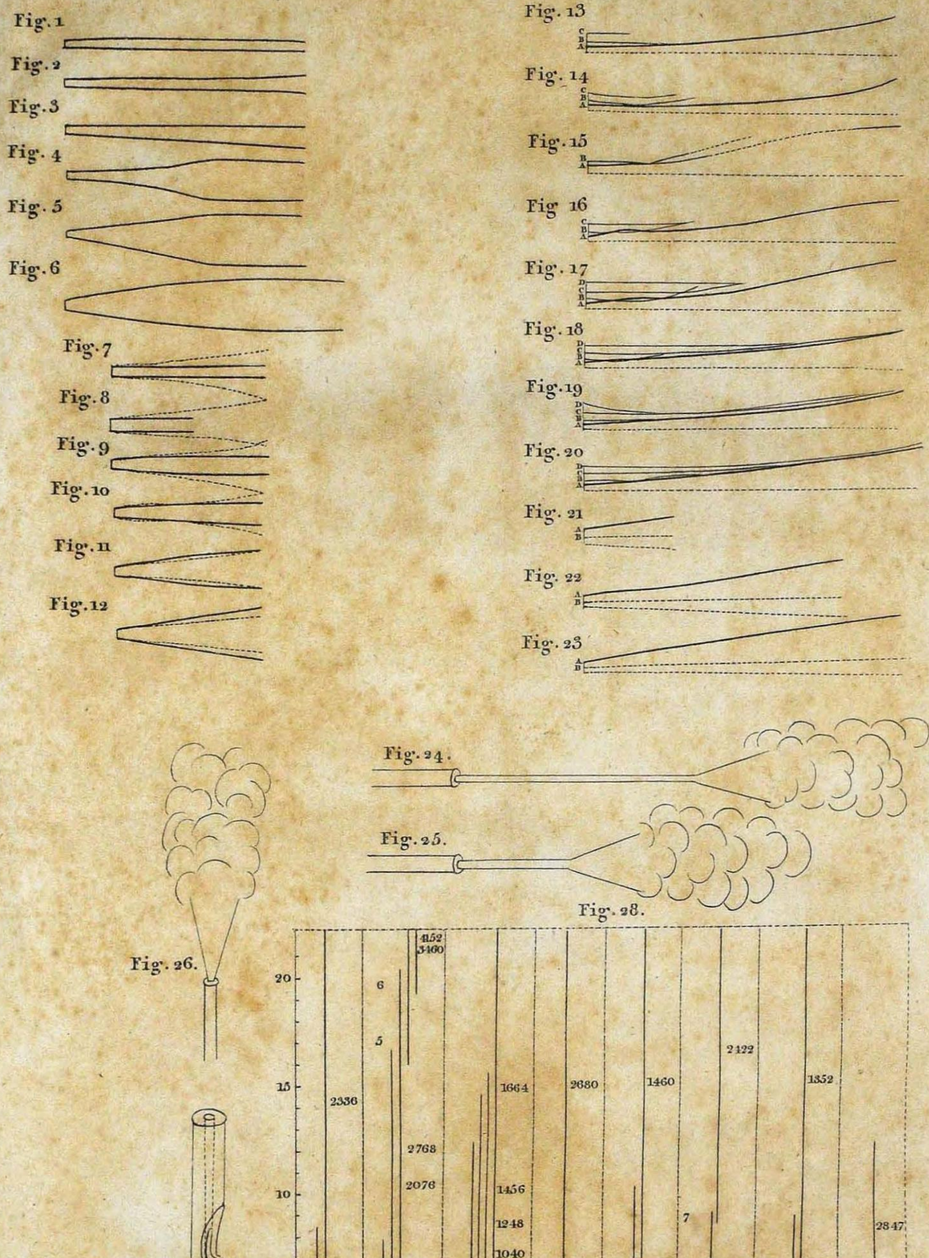


Fig. 4.



Dispersion & Intensity of solar radiant heat & Light

Fig. 5.



Dr. Young's Inquiries respecting Sound.